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TRACTS,

MATHEMATICAL AND PHYSICAL.

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BY

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TO

THE UNIVERSITY OF EDINBURGH,

These Tracts,

BEGUN WHILE ITS PUPIL,
FINISHED WHEN ITS HEAD,

ARE INSCRIBED BY

THE AUTHOR,

IN GRATEFUL REMEMBRANCE
OF BENEFITS CONFERRED OF OLD
AND HONOURS OF LATE BESTOWED.

PREFACE.

THESE Tracts were written at different times between 1796 and 1858. The first was inserted in the 'Philosophical Transactions,' with two other Papers on Light omitted in this collection. These three belong to the years 1794, 5, 6, and 7, when the author was a student at the University under Professors Playfair and Robison. He could have wished to insert an exercise which he gave in while at the class of the former in 1794, which Mr. Playfair was in the habit of showing, as having had the good fortune to hit upon the Binomial Theorem, but only by induction, as its author said in answer to the Professor's question, by what means he had arrived at it. He made inquiry some years ago, and found that the Professor's papers had not been preserved. But he cannot pass over this reminiscence of the University, nor a circumstance which upon the Professor's expression of an opinion respecting his pupil's good fortune, at once fixed his inclination for mathematical studies.

The Third Tract was believed to be required for elucidating D'Alembert's extension of the Integral Calculus, there being no distinct account anywhere of the history of that important step, nor indeed any very clear statement of its nature and limits.

The Eleventh and Twelfth Tracts may possibly prove useful to students of the Principia; at all events, they give the analytical treatment of the fundamental truths in the system—handled by Newton synthetically and with extreme conciseness, and therefore elliptically.

The Fourth Tract, on the Greek Geometry, it is hoped may have a tendency to encourage the study of the Ancient Analysis in conjunction with the modern, from which it is too often severed. The authority of M. Chasles is referred to in Note II. to this Tract, in favour of close attention to the Ancient Analysis. That he is far from undervaluing the modern is manifest; indeed, his work on the Higher Geometry sufficiently proves this; and he occupies the chair of Professor of that science, the first appointed since its establishment —an inestimable benefit bestowed upon mathematical science by the government of France. Let us hope that this our University will receive the same benefit from the government of our own country; a hope which may appear well grounded when we recollect that of its three most important members, one has been representative of Cambridge and pupil of

Stewart, another an alumnus of this University and pupil of Playfair, and a third our present Lord Rector, selected, not from any connexion whatever with our body, but as a testimony to his talents and learning.

No alteration has been made in any of these Tracts in preparing them for this work, except changing the fluxional for the differential notation. But the author has very carefully gone through all the analytical processes, in order to make sure that no error or oversight had occurred in investigations conducted at different times and in various circumstances. Hardly any were found, except typographical ones in former publications.

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ERRATA.

Page 4, line 5 from bottom—*for* neglecting *read* neglected.
„ 73, note—*for* Grandfather *read* Great-grandfather, *and for* father *read* grandfather.
„ 90, note, line 1—*after* lettre *insert a.*
„ 96, line 7 from bottom—*for* était *read* étant.
„ 96, line 6 from bottom—*for* or *read* ou.
„ 197, line 4 from bottom—*for* throught *read* throughout.
„ 226, last line—*for* subject investigates *read* subsequent investigations.
„ 265, line 6—*for* beyond *read* outside and without.
„ 298, line 4—*for* cardiordeal *read* cardioide.
„ 300, last line—*for* in *read m.*

INTRODUCTORY REMARKS.

It is not correct — it is the very reverse of the truth — to represent the practical applications of science as the only real, and, as it were, tangible profit derived from scientific discoveries or philosophical pursuits in general. There cannot be a greater oversight or greater confusion of ideas than that in which such a notion has its origin. It is near akin to the fallacy which represents profitable or productive labour as only that kind of labour by which some substantial or material thing is produced or fashioned. The labour which of all others most benefits a community, the superior order of labour which governs, defends, and improves a state, is by this fallacy excluded from the title of productive, merely because, instead of bestowing additional value on one mass or parcel of a nation's capital, it gives additional value to the whole of its property, and gives it that quality of security without which all other value would be worthless. So they who deny the importance of mere scientific contemplation, and exclude from the uses of science the pure and real pleasure of discovering, and of learning, and of surveying its truths, forget how many of the enjoyments derived from what are called the practical applications of the sciences, resolve themselves into gratifications of a merely contemplative kind. Thus, the steam engine is confessed to be the most useful application of machinery and of chemistry to the arts. Would it not be so if steam navigation were its only result, and if no one used

a steam-boat but for excursions of curiosity or of amusement? Would it not be so if steam-engines had never been used but in the fine arts? So a microscope is a useful practical application of optical science as well as a telescope—and a telescope would be so, although it were only used in examining distant views for our amusement, or in showing us the real figures of the planets, and were of no use in navigation or in war. The mere pleasure, then, of tracing relations, and of contemplating general laws in the material, the moral, and the political world, is the direct and legitimate value of science; and all scientific truths are important for this reason, whether they ever lend any aid to the common arts of life or no. In like manner the mental gratification afforded by the scientific contemplations of Natural Religion are of great value, independent of their much higher virtue in elevating the mind, mending the heart, and improving the life,—towards which important object, indeed, all contemplations of science more or less directly tend,—and in this higher sense all the pleasures of science are justly considered as its Practical Uses.

If it be a pleasure to gratify curiosity, to know what we were ignorant of, to have our feelings of wonder called forth, how pure a delight of this very kind does Natural Science hold out to its students! Recollect some of the extraordinary discoveries of Mechanical Philosophy. How wonderful are the laws that regulate the motions of fluids! Is there anything in all the idle books of tales and horrors more truly astonishing than the fact, that a few pounds of water may, by mere pressure, without any machinery—by merely being placed in a particular way, produce an irresistible force? What can be more strange, than that an ounce weight should balance hundreds of pounds, by the intervention of a few bars of thin iron? Observe the extraordinary truths which Optical Science discloses. Can anything surprise us more, than to find that the colour of white is a mixture of all others—that red, and blue, and green, and all the rest, merely by being blended in certain proportions, form what we had fancied rather to be no colour at all, than all colours together?

Chemistry is not behind in its wonders. That the diamond should be made of the same material with coal; that water should be chiefly composed of an inflammable substance; that acids should be, for the most part, formed of different kinds of air, and that one of those acids, whose strength can dissolve almost any of the metals, should consist of the self-same ingredients with the common air we breathe; that salts should be of a metallic nature, and composed, in great part, of metals, fluid like quicksilver, but lighter than water, and which, without any heating, take fire upon being exposed to the air, and by burning, form the substance so abounding in saltpetre and in the ashes of burnt wood: these, surely, are things to excite the wonder of any reflecting mind—nay, of any one but little accustomed to reflect. And yet these are trifling when compared to the prodigies which Astronomy opens to our view: the enormous masses of the heavenly bodies; their immense distances; their countless numbers, and their motions, whose swiftness mocks the uttermost efforts of the imagination.

Akin to this pleasure of contemplating new and extraordinary truths, is the gratification of a more learned curiosity, by tracing resemblances and relations between things, which, to common apprehension, seem widely different. Mathematical science to thinking minds affords this pleasure in a high degree. It is agreeable to know that the three angles of every triangle, whatever be its size, howsoever its sides may be inclined to each other, are always, of necessity, when taken together, the same in amount: that any regular kind of figure whatever, upon the one side of a right-angled triangle, is equal to the two figures of the same kind upon the two other sides, whatever be the size of the triangle: that the properties of an oval curve are extremely similar to those of a curve which appears the least like it of any, consisting of two branches of infinite extent, with their backs turned to each other. To trace such unexpected resemblances is, indeed, the object of all philosophy; and experimental science, in particular, is occupied with such investigations, giving us general

views, and enabling us to explain the appearances of nature, that is, to show how one appearance is connected with another. But we are now considering only the gratification derived from learning these things. It is surely a satisfaction, for instance, to know that the same thing, or motion, or whatever it is, which causes the sensation of heat, causes also fluidity, and expands bodies in all directions;—that electricity, the light which is seen on the back of a cat when slightly rubbed on a frosty evening, is the very same matter with the lightning of the clouds;—that plants breathe like ourselves, but differently by day and by night;—that the air which burns in our lamps enables a balloon to mount, and causes the globules of the dust of plants to rise, float through the air, and continue their race—in a word, is the immediate cause of vegetation. Nothing can at first view appear less like, or less likely to be caused by the same thing, than the processes of burning and of breathing,—the rust of metals and burning, —an acid and rust,—the influence of a plant on the air it grows in by night, and of an animal on the same air at any time, nay, and of a body burning in that air; and yet all these are the same operation. It is an undeniable fact, that the very same thing which makes the fire burn, makes metals rust, forms acids, and enables plants and animals to breathe; that these operations, so unlike to common eyes, when examined by the light of science are the same,—the rusting of metals,—the formation of acids,—the burning of inflammable bodies,—the breathing of animals,—and the growth of plants by night. To know this is a positive gratification. Is it not pleasing to find the same substance in various situations extremely unlike each other;—to meet with fixed air as the produce of burning, of breathing, and of vegetation;—to find that it is the choke-damp of mines, the bad air in the grotto at Naples, the cause of death in neglecting brewers' vats, and of the brisk and acid flavour of Seltzer and other mineral springs? Nothing can be less like than the working of a vast steam-engine, of the old construction, and the crawling of a fly upon the window. Yet we find that these two opera-

tions are performed by the same means, the weight of the atmosphere, and that a sea-horse climbs the ice-hills by no other power. Can anything be more strange to contemplate? Is there in all the fairy tales that ever were fancied anything more calculated to arrest the attention and to occupy and to gratify the mind, than this most unexpected resemblance between things so unlike to the eyes of ordinary beholders? What more pleasing occupation than to see uncovered and bared before our eyes the very instrument and the process by which Nature works? Then we raise our views to the structure of the heavens; and are again gratified with tracing accurate but most unexpected resemblances. Is it not in the highest degree interesting to find, that the power which keeps this earth in its shape, and in its path, wheeling upon its axis and round the sun, extends over all the other worlds that compose the universe, and gives to each its proper place and motion; that this same power keeps the moon in her path round our earth, and our earth in its path round the sun, and each planet in its path; that the same power causes the tides upon our globe, and the peculiar form of the globe itself; and that, after all, it is the same power which makes a stone fall to the ground? To learn these things, and to reflect upon them, occupies the faculties, fills the mind, and produces certain as well as pure gratification.

But if the knowledge of the doctrines unfolded by science is pleasing, so is the being able to trace the steps by which those doctrines are investigated, and their truth demonstrated: indeed you cannot be said, in any sense of the word, to have learnt them, or to know them, if you have not so studied them as to perceive how they are proved. Without this you never can expect to remember them long, or to understand them accurately; and that would of itself be reason enough for examining closely the grounds they rest on. But there is the highest gratification of all, in being able to see distinctly those grounds, so as to be satisfied that a belief in the doctrines is well founded. Hence to follow a demonstration of a great mathematical truth — to perceive

how clearly and how inevitably one step succeeds another, and how the whole steps lead to the conclusion—to observe how certainly and unerringly the reasoning goes on from things perfectly self-evident, and by the smallest addition at each step, every one being as easily taken after the one before as the first step of all was, and yet the result being something not only far from self-evident, but so general and strange, that you can hardly believe it to be true, and are only convinced of it by going over the whole reasoning—this operation of the understanding, to those who so exercise themselves, always affords the highest delight. The contemplation of experimental inquiries, and the examination of reasoning founded upon the facts which our experiments and observations disclose, is another fruitful source of enjoyment, and no other means can be devised for either imprinting the results upon our memory, or enabling us really to enjoy the whole pleasures of science. They who found the study of some branches dry and tedious at the first, have generally become more and more interested as they went on; each difficulty overcome gives an additional relish to the pursuit, and makes us feel, as it were, that we have by our work and labour established a right of property in the subject.

I.

GENERAL THEOREMS, CHIEFLY PORISMS, IN THE HIGHER GEOMETRY.*

THE following are a few propositions that have occurred to me in the course of a considerable degree of attention which I have happened to bestow on that interesting, though difficult branch of speculative mathematics, the higher geometry. They are all in some degree connected; the greater part refer to the conic hyperbola, as related to a variety of other curves. Almost the whole are of that kind called porisms, whose nature and origin is now well known; and, if that mathematician to whom we owe the first distinct and popular account of this formerly mysterious, but most interesting subject,† should chance to peruse these pages, he will find in them additional proofs of the accuracy which characterizes his inquiry into the discovery of this singularly-beautiful species of proposition.

Though each of the truths which I have here enunciated is of a very general and extensive nature, yet they are all discovered by the application of certain principles or properties still more general; and are thus only cases of propositions still more extensive. Into a detail of these I cannot at present enter: they compose a system of general methods, by which the discovery of propositions is effected with certainty and ease; and they are, very probably, in the doctrine of curve lines, what the ancients appear to have prized so much in plain geometry; though unfortunately all that remains to

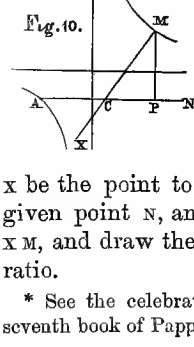
* From Phil. Trans., 1798, part ii.

† See Mr. Playfair's Paper in vol. iii. of the 'Edinburgh Transactions.'

us of that treasure is the knowledge of its high value. I have not added the demonstrations, which are all purely geometrical, granting the methods of tangents and quadratures: I have given an example in the abridged synthesis of what I consider as one of the most intricate. It is unnecessary to apologise any further for the conciseness of this tract. Let it be remembered, that were each proposition followed by its analysis and composition, and the corollaries, scholia, limitations, and problems, immediately suggested by it, without any trouble on the reader's part, the whole would form a large volume, in the style of the ancient geometers; containing the investigation of a series of connected truths, in one branch of the mathematics, all arising from varying the combinations of certain data enumerated in a general enunciation.*

As a collection of curious general truths, of a nature, so far as I know, hitherto unknown, I am persuaded that this paper, with all its defects, may not be unacceptable to those who feel pleasure in contemplating the varied and beautiful relations between abstract quantities, the wonderful and extensive analogies which every step of our progress in the higher parts of geometry opens to our view.

PROP. 1. *Porism.* Fig. 10. — A conic hyperbola being given, a point may be found, such, that every straight line drawn from it to the curve, shall cut, in a given ratio, that part of a straight line passing through a given point which is intercepted between a point in the curve not given, but which may be found, and the ordinate to the point where the first-mentioned line meets the curve.—Let x be the point to be found, N A the line passing through the given point N, and M any point whatever in the curve; join X M, and draw the ordinate M P; then A C is to C P in a given ratio.



* See the celebrated account of ancient geometrical works, in the seventh book of Pappus.

Corol. This property suggests a very simple and accurate method of describing a conic hyperbola, and then finding its centre, asymptotes, and axes; or, any of these being given, of finding the curve and the remaining parts.

PROP. 2. *Porism.*—A conic hyperbola being given, a point may be found, such, that if from it there be drawn straight lines to all the intersections of the given curve, with an infinite number of parabolas, or hyperbolas, of any given order whatever, lying between straight lines, of which one passes through a given point, and the other may be found; the straight lines so drawn, from the point found, shall be tangents to the parabolas, or hyperbolas.—This is in fact two propositions; there being a construction for the case of parabolas, and another for that of hyperbolas.

PROP. 3. *Porism.*—If, through any point whatever of a given ellipse, a straight line be drawn parallel to the conjugate axis, and cutting the ellipse in another point; and if at the first point a perpendicular be drawn to the parallel; a point may be found, such, that if from it there be drawn straight lines, to the innumerable intersections of the ellipse with all the parabolas of orders not given, but which may be found, lying between the lines drawn at right angles to each other, the lines so drawn from the point found, shall be normals to the parabolas at their intersections with the ellipse.

PROP. 4. *Porism.*—A conic hyperbola being given, if through any point of it a straight line be drawn parallel to the transverse axis, and cutting the opposite hyperbolas, a point may be found, such, that if from it there be drawn straight lines, to the innumerable intersections of the given curve with all the hyperbolas of orders to be found, lying between straight lines which may be found, the straight lines so drawn shall be normals to the hyperbolas at the points of section.

Scholium. The last two propositions give an instance of the many curious and elegant analogies between the hyperbola and ellipse; failing however when, by equating the axes, we change the ellipse into a circle.

PROP. 5. *Local Theorem.* Fig. 11.—If from a given point A, a straight line DE move parallel to itself, and another CS, from a given point C, move along with it round C; and a point I move along AB, from H, the middle point of AB, with a velocity equal to half the velocity of DE; then, if AP be always taken a third proportional to AS and BC, and through P, with asymptotes D′E′ and AB, a conic hyperbola be described; also with focus I and axis AB, a conic parabola be described; then the radius vector from C to M, the intersection of the two curves, moving round C, shall describe a given ellipse.

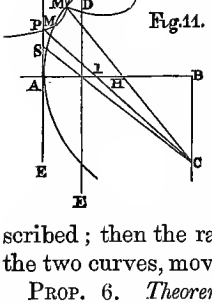


Fig. 11.

PROP. 6. *Theorem.*—A common logarithmic being given, and a point without it, a parabola, hyperbola, and ellipse may be described, which shall intersect the logarithmic and each other in the same points; the parabola shall cut the logarithmic orthogonally; and if straight lines be drawn from the given point to the common intersections of the four curves, these lines shall be normals to the logarithmic.

PROP. 7. *Porism.*—Two points in a circle being given, but not in one diameter, another circle may be described, such, that if from any point of it to the given points straight lines be drawn, and a line touching the given circle, the tangent shall be a mean proportional between the lines so inflected. Or, more generally, the square of the tangent shall have a given ratio to the rectangle under the inflected lines.

PROP. 8. *Porism.* Fig. 12.—Two straight lines AB, AP, not parallel, being given in position, a conic parabola MN may be found, such, that if, from any point of it M, a perpendicular MP be drawn to the one of the given lines nearest the curve, and this perpendicular be produced till it meets the other line in B; and if from B a line be drawn to a given point C; then MP shall have to PB together with CB, a given ratio.

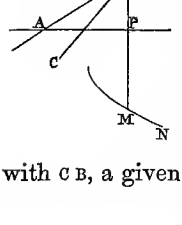
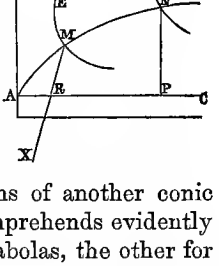


Fig. 12.

Scholium. This is a case of a more general enunciation, which gives rise to an infinite variety of the most curious porisms.

PROP. 9. *Porism.* Fig. 13.—A conic hyperbola being given, a point may be found, from which if straight lines be drawn to the intersections of the given curve with innumerable parabolas, or hyperbolas, of any given order whatever lying between perpendiculars which meet in a given point, the lines so drawn shall cut, in a given ratio, all the areas of the parabolas or hyperbolas contained by the peripheries and co-ordinates to points of it, found by the innumerable intersections of another conic hyperbola, which may be found.—This comprehends evidently two propositions; one for the case of parabolas, the other for that of hyperbolas. In the former it is thus expressed with a figure. Let EM be the given hyperbola; BA, AC, the perpendiculars meeting in a given point A: a point X may be found, such, that if XM be drawn to any intersection M of EM with any parabola AMN, of any given order whatever, and lying between AB and AC, XM shall cut, in a given ratio, the area AMNP, contained by AMN and AP, PN, co-ordinates to the conic hyperbola FN, which is to be found; thus, the area ARM shall be to the area RMNP in a given ratio.



PROP. 10. *Porism.*—A conic hyperbola being given, a point may be found, such, that if from it there be drawn straight lines, to the innumerable intersections of the given curve with all the straight lines drawn through a given point in one of the given asymptotes, the first-mentioned lines shall cut, in a given ratio, the areas of all the triangles whose bases and altitudes are the co-ordinates to a second conic hyperbola, which may be found, at the points where it cuts the lines drawn from the given point.

PROP. 11. *Porism.*—A conic hyperbola being given, a straight line may be found, such, that if another move along it in a given angle, and pass through the intersections of the

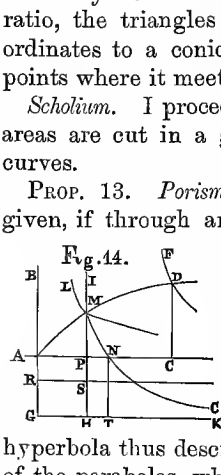
curve with all the parabolas, or hyperbolas, of any given order whatever, lying between straight lines to be found, the moving line shall cut, in a given ratio, the areas of the curves described, contained by the peripheries and co-ordinates to another conic hyperbola, that may be found, at the points where this cuts the curves described.

PROP. 12. *Porism.*—A conic hyperbola being given, a straight line may be found, along which if another move in a given angle, and pass through any point whatever of the hyperbola, and if this point of section be joined with another that may be found, the moving line shall cut, in a given ratio, the triangles whose bases and altitudes are the co-ordinates to a conic hyperbola, which may be found, at the points where it meets the lines drawn from the point found.

Scholium. I proceed to give one or two examples, wherein areas are cut in a given ratio, not by straight lines, but by curves.

PROP. 13. *Porism.* Fig. 14.—A conic hyperbola being given, if through any of its innumerable intersections with all the parabolas of any order, lying between straight lines, of which one is an asymptote, and the other may be found; an hyperbola of any order be described, except the conic, from a given origin in the given asymptote perpendicular to the axis of the parabolas, the hyperbola thus described shall cut, in a given ratio, an area, of the parabolas, which may be always found.

Fig. 14.



If from G, as origin, in AB, one of LM's asymptotes, there be described an hyperbola IC′, of any order whatever, except the first, and passing through M, a point where LM cuts any of the parabolas AM, of any order whatever, drawn from A a point to be found, and lying between AB and AC, an area ACD may be always found (that is, for every case of AM and IC′), which shall be constantly cut by IC′, in the given ratio of M : N; that is, the area AMN : NMDC :: M : N. I omit the analysis, which leads to the following construction and composition.

Constr. Let $m+n$ be the order of the parabolas, and $p+q$ that of the hyperbolas. Find ϕ a 4th proportional to $m+n$, $q-p$ and $m+2n$; divide GB in A, so that AR : AG :: $q : p+\phi$; then find π a 4th proportional to M + N, $\phi+p$, and $q-p$, and γ a 4th proportional to q, AG, and $q-p$; and, lastly, θ a 4th proportional to the parameter* of LM, π and M. If, with a parameter equal to $\frac{m+n}{m+2n} \times \theta - \frac{M+N}{M}$ of the rectangle τ . AN, and between the asymptotes AB, AC, a conic hyperbola be described, it shall cut the parabola in a point, the co-ordinates to which contain an area that shall be cut by IC' in the ratio of M : N.

Demonstration. Because AG is divided in R, so that AR : AG :: $q : p+\phi$, and that $\phi : m+n :: q-p : m+2n$, AR is equal to $p + \frac{AG \times q}{\frac{(m+n) \times (q-p)}{m+2n}}$; and, because LM is a conic hyperbola, the rectangle MS . RS, or MS . AP, or AP . (MP + AR) is equal to the parameter, or constant space, therefore this parameter is equal to AP $\times \left(MP + p + \frac{AG \,.\, q}{\frac{(m+n) \,.\, (q-p)}{m+2n}} \right)$.

Again, the space ACD is equal to $\frac{m+n}{m+2n}$ of the rectangle AC . CD, since AD is a parabola of the order $m+n$; but by construction AC . CD is equal to $\frac{m+n}{m+2n}$ of $\left(\theta - \frac{M+N}{M} \,.\, \tau \,.\, AN \right)$; therefore, ACD $= \theta - \frac{M+N}{M} \,.\, \tau \,.\, AN$, of which θ : parameter of LM :: π : M, and π : M + N :: $\phi + p : q - p$; therefore $\theta = \frac{\text{Par. LM} \times (M+N)}{M\,(q-p)} \times \left(\frac{(m+n) \times (q-p)}{m+2n} + p \right)$; also $\tau : q$ ::

* i.e., the constant rectangle or space to which AP . SM is equal.

AG : $q-p$; consequently ACD = $\frac{\text{Par. LM} \times (\text{M}+\text{N})}{\text{M}\,(q-p)}$ multiplied by $\left(\frac{m+n \times q-p}{m+2n}+p\right)$ and diminished by $\frac{\text{M}+\text{N}}{\text{M}} \times \text{AN} \times \frac{q \cdot \text{AG}}{q-p}$; therefore, transposing $\frac{\text{Par. LM} \times (\text{M}+\text{N})}{\text{M} \times (q-p)} \times \left(\frac{m+n}{m+2n} \times \frac{q-p}{1}+p\right)$ is equal to $\text{ACD} + \frac{\text{M}+\text{N}}{\text{M}} \times \text{AN} \times \frac{q \cdot \text{AG}}{q-p}$; and par. LM will be equal to

$$\frac{\left(\text{ACD} + \frac{\text{M}+\text{N}}{\text{M}} \times \text{AN} \times \frac{q \cdot \text{AG}}{q-p}\right) \times \frac{\text{M}}{q-p}}{\left(\frac{m+n}{m+2n} \times \frac{q-p}{1}+p\right) \times (\text{M}+\text{N})}, \text{ that is,}$$

$$\frac{\frac{\text{M}}{\text{M}+\text{N}} \times (q-p) \times \text{ACD} + q \cdot \text{AN} \times \text{AG}}{\frac{m+n}{m+2n} \times (q-p)+p}.$$

Now it was before demonstrated, that the parameter of LM is equal to $\text{AP} \times \left(\text{MP}+p+\frac{q \cdot \text{AG}}{\frac{(p+n) \cdot (q-p)}{m+2n}}\right)$. This is therefore equal to $\frac{\frac{\text{M}}{\text{M}+\text{N}} \times (q-p) \times \text{ACD} + q \cdot \text{AN} \times \text{AG}}{\frac{m+n}{m+2n} \times (q-p)+p}$, multiplying both by $\frac{(m+n) \times (q-p)}{m+2n}+p$, we have $\frac{\text{M}}{\text{M}+\text{N}} \times (q-p) \times \text{ACD} + q \cdot \text{AN} \times \text{AG} = \text{AP} \times (\text{MP} \times (p + \left(\frac{(m+n) \times (q-p)}{m+2n}\right)+q \cdot \text{AG})$.

From these equals take $q \cdot \text{AG} \times \text{AN}$, and there remains $\frac{\text{M}}{\text{M}+\text{N}} \times (q-p) \times \text{ACD}$ equal to $\text{AP} \times \text{PM} \times \left(\frac{(m+n) \times (q-p)}{m+2n}\right.$

$+p\Big) + q$. AG $\times$ (AP $-$ AN); or, dividing by $q-p$, $\frac{\text{M}}{\text{M}+\text{N}}$ $\times$ ACD $=$ AP $\times \left(\frac{m+n}{m+2n}\right) + \left(\frac{p}{q-p}\right) \times$ PM $+ \frac{q}{q-p} \times$ AG $\times$ (AP $-$ AN). Now, $\frac{m+n}{m+2n} \times$ AP $\times$ PM is equal to the area APM; therefore the area APM together with $\frac{p}{q-p} \times$ AP . PM, and $\frac{q}{q-p} \times$ AG $\times$ (AP $-$ AN), or APM with $\frac{p}{q-p} \times$ AP . PM $- \frac{q}{q-p} \times$ AG $\times$ (AN $-$ AP), or APM $+ \frac{q}{q-p} \times$ AP . PM $- \frac{q}{q-p} \times$ rect. PT, is equal to $\frac{\text{M}}{\text{M}+\text{N}} \times$ ACD. Now IC′ is an hyperbola of the order $p+q$; therefore its area is $\frac{p}{p-q} \times$ rect. GH . MH. But q is greater than p; therefore $\frac{p}{p-q}$ is negative, and $\frac{p \times \text{GH . HM}}{q-p}$ is the area MHKC′; and the area NTKC′ is equal to $\frac{p}{q-p} \times$ GT $\times$ TN; therefore MNTH is equal to (MHKC′ $-$ NTKC′), or to $\frac{p}{q-p} \times$ (GH . MH $-$ GT . TN). From these equals take the common rectangle AT, and there remains the area MPN, equal to $\frac{p}{q-p} \times$ AP $\times$ MP $- \frac{q}{q-p} \times$ PT; which was before demonstrated to be, together with APM, equal to $\frac{\text{M}}{\text{M}+\text{N}}$. ACD. Therefore MPN, together with APM, that is, the area AMN, is equal to $\frac{\text{M}}{\text{M}+\text{N}}$. ACD; consequently AMN : ACD :: M : M $+$ N; and (dividendo) AMN : NMDC :: M : N. An area has therefore been found, which the hyperbola IC′ always cuts in a given ratio. Therefore, a conic hyperbola being given, &c. Q. E. D.

Scholium. This proposition points out, in a very striking manner, the connexion between all parabolas and hyperbolas, and their common connexion with the conic hyperbola. The demonstration here given is much abridged; and, to avoid circumlocution, algebraic symbols, and even ideas, have been introduced: but by attending to the several steps, any one will easily perceive that it may be translated into geometrical language, and conducted on purely geometrical principles, if any numbers be substituted for m, n, p, and q; or if these letters be made representatives of lines, and if conciseness be less rigidly studied.

PROP. 14. *Theorem.*—A common logarithmic being given; if from a given point, as origin, a parabola, or hyperbola, of any order whatever be described, cutting in a given ratio a given area of the logarithmic; the point where this curve meets the logarithmic is always situated in a conic hyperbola, which may be found.

Scholium. This proposition is, properly speaking, neither a porism, a theorem, nor a problem. It is not a theorem, because something is left to be found, or, as Pappus expresses it, there is a deficiency in the hypothesis: neither is it a porism; for the theorem, from which the deficiency distinguishes it, is not local.

PROP. 15. *Porism.* Fig. 15.—A conic hyperbola being given; two points may be found, from which if straight lines be inflected, to the innumerable intersections of the given curve with parabolas or hyperbolas, of any given order whatever, described between given straight lines; and if co-ordinates be drawn to the intersections of these curves with another conic hyperbola, which may be found; the lines inflected shall always cut off areas that have to one another a given ratio, from the areas contained by the co-ordinates.—Let X and Y be the points found; HD the given hyperbola, FE the one to be found; ADC one of the curves lying between AB and AG, intersecting HD and FE; join XD, YD; then the area AYD : XDCB in a given ratio.

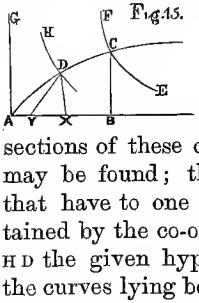


Fig. 15.

PROP. 16. *Porism.* Fig. 16.—If between two straight lines making a right angle, an infinite number of parabolas of any order whatever be described; a conic parabola may be drawn, such, that if tangents be drawn to it at its intersections with the given curves, these tangents shall always cut, in a given ratio, the areas contained by the given curves, the curve found, and the axis of the given curves.—Let AMN be one of the given parabolas; DMO the parabola found, and TM its tangent at M: ATM shall have to TMR a given ratio.

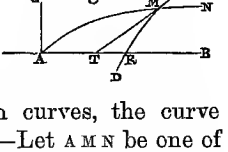


Fig. 16.

PROP. 17. *Porism.*—A parabola of any order being given; two straight lines may be found, between which if innumerable hyperbolas of any order be described; the areas cut off by the hyperbolas and the given parabola at their intersections, shall be divided, in a given ratio, by the tangents to the given curve at the intersections; and conversely, if the hyperbolas be given, a parabola may be found, &c.

PROP. 18. *Porism.*—A parabola of any order $(m + n)$ being given, another of an order $(m + 2n)$ may be found, such, that the rectangle under its ordinate and a given line, shall have always a given ratio to the area (of the given curve) whose abscissa bears to that of the curve found a given ratio.

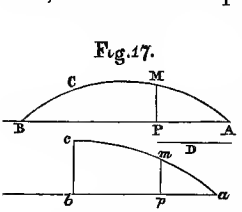
Example. Let $m = 1$, $n = 1$, and let the given ratios be those of equality; the proposition is this: a conic parabola being given, a semi-cubic one may be found, such, that the rectangle under its ordinate and a given line, shall be always equal to the area of the given conic parabola, at equal abscissæ.

Scholium. A similar general proposition may be enunciated and exemplified, with respect to hyperbolas; and as these are only cases of a proposition applying to all curves whatever, I shall take this opportunity of introducing a very simple, and I think perfectly conclusive demonstration, of the 28th lemma, "Principia," Book i., "that no oval can be squared." It is well known, that the demonstration which Sir Isaac Newton gives of this lemma is not a little intricate; and,

whether from this difficulty, or from some real imperfection, or from a very natural wish not to believe that the most celebrated desideratum in geometry must for ever remain a desideratum, certain it is, that many have been inclined to call in question the conclusiveness of that proof.

Let AMC be any curve whatever (fig. 17), and D a given line; take in ab a part ap, having to AP a given ratio, and erect a perpendicular pm, such, that the rectangle pm . D shall have to the area APM a given ratio; it is evident that m will describe a curve amc, which can never cut the axis, unless in a. Now because pm is proportional to $\frac{\text{APM}}{\text{D}}$, or to APM, pm will always increase ad infinitum, if AMC is infinite; but if AMC stops or returns into itself, that is, if it is an oval, pm is a maximum at b, the point of ab corresponding to B in AB; consequently the curve amc stops short, and is irrational. Therefore pm, its ordinate, has not a finite relation to ap, its abscissa; but ap has a given ratio to AP; therefore pm has not a finite relation to AP, and APM has a given ratio to pm; therefore it has not a finite relation to AP, that is, APM cannot be found in finite terms of AP, or is incommensurate with AP; therefore the curve AMB cannot be squared. Now AMB is any oval; therefore no oval can be squared. By an argument of precisely the same kind, it may be proved, that the rectification, also, of every oval is impossible. Therefore, &c. Q. E. D.

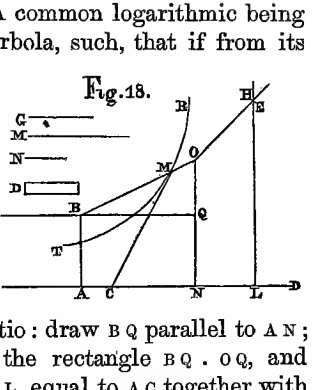
Fig. 17.



I shall subjoin three problems, that occurred during the consideration of the foregoing propositions. The first is an example of the application of the porisms to the solution of problems. The second gives, besides, a new method of resolving one of the most celebrated ever proposed, Kepler's problem; and the last exhibits a curve before unknown, at least to me, as possessing the singular property of a constant tangent.

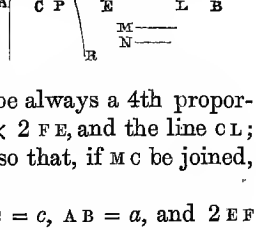
PROP. 19. *Problem.* Fig. 18.—A common logarithmic being given; to describe a conic hyperbola, such, that if from its intersection with the given curve a straight line be drawn to a given point, it shall cut a given area of the logarithmic in a given ratio. The analysis leads to this construction. Let BME be the logarithmic, G its modula; AB the ordinate at its origin A; let C be the given point; ANOB the given area; M : N the given ratio: draw BQ parallel to AN; find D a 4th proportional to M, the rectangle BQ . OQ, and M + N. From AD cut off a part AL, equal to AC together with twice G; at L make LH perpendicular to AD, and between the asymptotes AL, HL, with a parameter, or constant rectangle, twice (D + 2 . AB . G) describe a conic hyperbola; it is the curve required.

Fig. 18.



PROP. 20. *Problem.* Fig. 19.—To draw, through the focus of a given ellipse, a straight line that shall cut the area of the ellipse in a given ratio.—*Const.* Let AB be the transverse axis, EF the semi-conjugate; E, of consequence, the centre; C and L the foci. On AB describe a semicircle. Divide the quadrant AK in O in the given ratio of M to N, in which the area is to be cut, and describe the cycloid GMR, such, that the ordinate PM may be always a 4th proportional to the arc OQ, the rectangle AB × 2 FE, and the line CL; this cycloid shall cut the ellipse in M, so that, if MC be joined, the area ACM shall be to CMB :: M : N.

Fig. 19.



Demonstr. Let AP = x, PM = y, AC = c, AB = a, and 2 EF = b; then, by the nature of the cycloid GMR, — PM : OQ :: 2 FE × AB : CL, and QO = AO — AQ = by const. $\frac{M}{M+N}$ × (AK — AQ); also, CL = AB — 2 AC, since AC = LB. There-

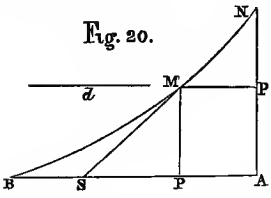
fore, $-\text{PM} : \frac{\text{M}}{\text{M}+\text{N}} \times \text{AK} - \text{AQ} :: \text{AB} \times 2\text{EF} : \text{AB} - 2\text{AC}$; or $-y : \frac{\text{M}}{\text{M}+\text{N}} \times \text{arc } 90° - \text{arc vers. sin. } x :: ab : a - 2c$; therefore $-y(a-2c)$ or $+y(2c-a) = ab \times \left(\frac{\text{M}}{\text{M}+\text{N}} \times \text{arc } . 90° - \text{arc v. s. } x\right)$, and by transposition $ab \times \text{arc } . \text{ v. s. } x + y(2c-a) = \frac{ab \,.\, \text{M}}{\text{M}+\text{N}} \times \text{arc } 90°$. To these equals add $2y(x-x) = 0$, and multiply by -1; then will $ab \times \text{arc v. s. } x + (2x-a)y - 2y(x-c) = \frac{\text{M}}{\text{M}+\text{N}} \times ab \text{ arc } 90°$, of which the 4th parts are also equal; therefore $\frac{ab \times \text{arc v. s. } x}{4} + \frac{(2x-a)y}{4} - \frac{y}{2}(x-c) = \frac{ab}{4} \times \frac{\text{M}}{\text{M}+\text{N}} \times \text{arc } 90°$. Now because AFB is an ellipse, $y^2 = \frac{b^2}{a^2} \times (ax - x^2)$, and $y = \frac{b}{a}\sqrt{(ax-x^2)}$; therefore $\frac{ab \times \text{arc v. s. } x}{4} + \frac{2x-a}{4} \times \frac{b}{a}\sqrt{(ax-x^2)} - \frac{y}{2}(x-c) = \frac{ab}{4} \times \frac{\text{M}}{\text{M}+\text{N}} \times \text{arc } 90°$. Multiply both numerator and denominator of the first and last terms by a; then will $\frac{b}{a} \times \frac{a^2}{4} \times \text{arc v. s. } x + \frac{2x-a}{4} \times \frac{b}{a}\sqrt{(ax-x^2)} - \frac{y}{2}(x-c) = \frac{b}{a} \times \frac{a^2}{4} \times \frac{\text{M}}{\text{M}+\text{N}} \times \text{arc } 90°$. Now the differential of an arc whose versed sine is x and radius $\frac{a}{2}$, is equal to $\frac{a\,dx}{2\sqrt{(ax-x^2)}}$, which is also the differential of the arc whose sine is $\sqrt{\frac{x}{a}}$ and radius unity; therefore $\frac{b}{a} \times \left(\frac{a^2}{4} \times \text{arc}\right.$

$\sin \sqrt{\frac{x}{a} + \frac{2x-a}{4} \times \sqrt{(ax - x^2)}} - \frac{y}{2}(x-c)$ is equal to $\frac{b}{a} \times \frac{a}{4} \times \frac{\text{M}}{\text{M}+\text{N}} \times$ arc 90°; and, by the quadrature of the circle, $\frac{a^2}{4} \times$ arc sin. $\sqrt{\frac{x}{a} + \frac{2x-a}{4} \times \sqrt{(ax - x^2)}}$, is the area whose abscissa is x; consequently the semicircle's area is $\frac{a^2}{4} \times$ arc 90°. But the areas of ellipses are to the corresponding areas of the circles described on their transverse axes, as the conjugate to the transverse; therefore $\frac{b}{a} \times \left(\frac{a^2}{4} \times \text{arc sin.} \sqrt{\frac{x}{a} + \frac{2x-a}{4} \times \sqrt{(ax - x^2)}}\right)$ is the area whose abscissa is x, of a semi-ellipse, whose axes are a and b; and consequently $\frac{b}{a} \times \frac{a^2}{4} \times$ arc 90° is the area of the semi-ellipse. Therefore the area APM $- \frac{y}{2}(x-c)$ is equal to $\frac{\text{M}}{\text{M}+\text{N}}$ of AMFB. But $\frac{y}{2}(x-c) = \frac{\text{PM}}{2} \times (\text{AP} - \text{AC}) = \frac{\text{PM}^2}{2} \times \text{PC}$, is the triangle CPM; consequently, APM $-$ CPM, or ACM, is equal to $\frac{\text{M}}{\text{M}+\text{N}} \times$ AMFB; and ACM : AMFB :: M : M + N; or (dividendo) ACM : CMFB :: M : N; and the area of the ellipse is cut in a given ratio by the line drawn through the focus. Q. E. D.

Of this solution it may be remarked, that it does not assume as a postulate the description of the cycloid; but gives a simple construction of that curve, flowing from a curious property, by which it is related to a given circle. This cycloid, too, gives, by its intersection with the ellipse, the point required, directly, and not by a subsequent construction, as Sir Isaac Newton's does. I was induced to give the demonstration, from a conviction that it is a good instance of the superiority of modern over ancient analysis; and in itself perhaps no inelegant specimen of algebraic demonstration.

PROP. 21. *Problem.* Fig. 20.—To find the curve whose tangent is always of the same magnitude.

Fig. 20.



Analysis. Let MN be the curve required, AB the given axis, SM a tangent at any point M, and let a be the given magnitude; then, $SM \cdot q. = SP \cdot q. + PM \cdot q. = a^2$; or, $y^2 + \frac{y^2 dx^2}{dy^2} = a^2$; and $\frac{dx^2}{dy^2} = \frac{a^2 - y^2}{y^2}$; therefore, $dx = \frac{dy}{y} \times \sqrt{a^2 - y^2}$. In order to integrate this equation, divide $\frac{dy}{y}\sqrt{a^2 - y^2}$ into its two parts, $\frac{a^2 dy}{y\sqrt{a^2 - y^2}}$ and $\frac{-y\,dy}{\sqrt{a^2 - y^2}}$; to find the integral of the former,

$$\frac{a^2 dy}{y\sqrt{a^2 - y^2}} = \frac{a^2 dy}{y} \times \frac{1 + \frac{a}{\sqrt{a^2 - y^2}}}{a + \sqrt{a^2 - y^2}} = -a \times$$

$$\times \left(\frac{-\frac{a\,dy}{y^2} - \frac{a^2 dy}{y^2\sqrt{a^2 - y^2}}}{\frac{a + \sqrt{a^2 - y^2}}{y}} \right) = -\frac{a \times \text{differential of} \left(\frac{a + \sqrt{a^2 - y^2}}{y} \right)}{\frac{a + \sqrt{a^2 - y^2}}{y}};$$

therefore the integral of $\frac{a^2 dy}{y\sqrt{a^2 - y^2}}$ is $-a \times$ hyp. log. $\frac{a + \sqrt{a^2 - y^2}}{y}$, and the integral of the other part, $\frac{-y\,dy}{\sqrt{a^2 - y^2}}$, is $+\sqrt{a^2 - y^2}$; therefore the integral of the aggregate $\frac{dy}{y}\sqrt{a^2 - y^2}$, is $\sqrt{a^2 - y^2} - a \times$ h. l. $\frac{a + \sqrt{a^2 - y^2}}{y}$, or

$\sqrt{a^2 - y^2} + a \times \text{h. l.} \dfrac{y}{a + \sqrt{a^2 - y^2}}$; a final equation to the curve as required. Q. E. I.

I shall throw together, in a few corollaries, the most remarkable things that have occurred to me concerning this curve.*

Corol. 1. The subtangent of this curve is $\sqrt{(a^2 - y^2)}$.

Corol. 2. In order to draw a tangent to it, from a given point without it; from this point as pole, with radius equal to a, and the curve's axis as directrix, describe a concoid of Nicomedes: to its intersections with the given curve draw straight lines from the given point; these will touch the curve.

Corol. 3. This curve may be described, organically, by drawing one end of a given flexible line or thread along a straight line, while the other end is urged by a weight towards the same straight line. It is consequently the curve of traction to a straight line.

Corol. 4. In order to describe this curve from its equation; change the one given above, by transferring the axes of its co-ordinates: it becomes (y being = P′M and x = AP′), $y = \sqrt{(a^2 - x^2)} + a \times \text{h. l.} \dfrac{x}{a + \sqrt{(a^2 - x^2)}}$; which may be used with ease, by changing the hyberbolic into the tabular logarithm. Thus, then, the common logarithmic has its subtangent constant; the conic parabola, its subnormal; the circle, its normal; and the curve which I have described in this proposition, its tangent.†

* There are other properties of this curve noted in Tract V. of this volume.

† This Tract was printed in Phil. Trans. for 1798, part 2. The fluxional notation has alone been altered to the differential.—The schol., p. 17, is subject to doubt from the leminscata and other similar curves. See note I. at end of this volume.—The subject of Porisms is treated of in Note II.

II.

KEPLER'S PROBLEM.

KEPLER was led, after the discovery of the law which bears his name, to the celebrated problem which also bears it. Having proved that the squares of the periodic times are as the cubes of the distances, he wished to discover a method of finding the true place of a planet at a given time—one of the most important and general problems in astronomy. By a short and easy process of reasoning, he reduced this question to the solution of a transcendental problem;—to draw from a given eccentric point, in the transverse of an ellipse (or the diameter of a circle) a straight line, which shall cut the area of the curve in a given ratio; or, in the language of astronomers, "from the given mean anomaly, to find the anomaly of the eccentric."

This most important problem is evidently transcendental; for, in the first place, the curve in question is not quadrable in algebraic terms; and, in the next place, admitting that it were, the solution cannot be obtained in finite terms. As the general question, for all trajectories, is of vast importance; and as the paper of Mr. Ivory, in the 'Edinburgh Transactions,' contains a most successful application of the utmost resources of algebraic skill to the most important case of it, I shall premise a few remarks upon the problem, when enunciated in different cases.

Let D^2 be the given area of any curve, whch is the trajectory of a planet or other body, or which is to be cut in the

given ratio of m to n. Let x and y, as usual, be the abscissa and ordinate, and c the eccentricity of the given point, through which the *radius vector* is to be drawn, if the equation is taken from the centre; or, if it is taken from the vertex, let c be the distance of the given point from that vertex, as the focal distance in the case of the planets or comets (supposing the comets to revolve round the sun in parabolas or eccentric ellipses, having the sun in the focus), then, it may easily be found, that the following differential equation $2\int y\,dx + y(c - x) = \frac{2m\,D^2}{m+n}$, if resolved for the case of any given curve, gives a solution of the problem for that curve. Instead of $\int y\,dx$, there must be substituted the general expression for the area found by integration; and y must then be expressed through the whole equation in terms of x, or x in terms of y: There will result an equation to x, or to y, which, when resolved, gives a solution of the problem.

Now, it is manifest, that one or both of two difficulties or impossibilities may occur in this investigation of the value of x. It may be impossible to exhibit $\int y\,dx$ in finite terms; and it may be impossible, even after finding $\int y\,dx$, to resolve the equation that results from substituting the value of $\int y\,dx$ in the general equation above given. Thus, if the given curve is not quadrable, the equation can never be resolved; but, although the curve is quadrable, it does not follow that the equation can be resolved.

In the case of the *circle* and *ellipse*, both these difficulties must of course occur. The value of $\int y\,dx$ in the circle being $\int dx\sqrt{ax - x^2}$, and in the ellipse $\int \frac{b\,dx}{a}\sqrt{ax - x^2}$ (where a and b are the transverse and conjugate), neither of which differentials can be integrated in finite terms, the general equations become indefinite or unintegrable.

The *lemniscata* (a curve of the fourth order) is quadrable in algebraic terms: but the resolution of our general equation cannot, in this case, be performed in finite terms; it leads to

an equation of the sixth order, very complicated and difficult.* But, if the given point is in the centre or *punctum duplex* of the curve, the equation is a cubic one, wanting the second term, and of course, easily resolved.

It often happens, too, that the problem may be resolved, in general, for a curve; but that, in one particular part of the axis, the solution becomes impossible. As this is rather a singular circumstance, we shall attend a little more minutely to it.

Let it be required to resolve the problem for the case of comets, supposing those bodies to move in parabolic orbits. The general equation for x becomes $x\sqrt{x} + 3c\sqrt{x} = \frac{6}{\sqrt{a}} \times \frac{m\,\mathrm{D}^2}{m+n}$; a cubic wanting the second term, and easily resolved. But, in certain cases, viz., when c, the distance of the given point from the vertex, is less than $3\,\mathrm{D} \times \sqrt[3]{\frac{m^2\,\mathrm{D}^2}{4\,a\,(m+n)^2}}$ the problem cannot be resolved; for, in this case, the cube of one-third of the co-efficient of x is less than the square of half the last term, which is the well-known irreducible case of

* The equation is of the following form, a being the lemniscata's semi-diameter :—

$$\left.\begin{aligned} &+ x^6 \\ &+ 6c\,(1 - a)\,x^5 \\ &+ \left(9c^2\,(1 + a^2 - 2a) - a^2\right)x^4 \\ &- 6c\,a^2\,(1 - a^2)\,x^3 \\ &+ \left(3a^4 - 9c^2a^2\,(1 + a^2 - 2a)\right)x^2 \\ &+ 6c\,a^4\,(1 - a)\,x \end{aligned}\right\} = \frac{12\,m}{m+n}\,a^2\,\mathrm{D}^2\left(a^2 - \frac{3\,m\,\mathrm{D}^2}{m+n}\right)$$

a cubocubic having all its terms ($x^6 + \mathrm{A}\,x^5 + \mathrm{B}\,x^4 + \mathrm{C}\,x^3 + \mathrm{D}\,x^2 + \mathrm{E}\,x + \mathrm{F} = 0$), in which A, C, and E vanish when the centre of motion (or of the *radii vectores*) is in the *punctum duplex*, and then the equation to x is $x^6 + \mathrm{B}\,x^4 + \mathrm{D}x^2 + \mathrm{F} = 0$, reducible to the cubic $z^3 + \Delta\,z + \phi = 0$. So that the problem is soluble, except when the eccentricity is such that $\left(\frac{\Delta}{3}\right)^3$ is less than $\left(\frac{\phi}{2}\right)$, the irreducible case of Cardan's rule.

Cardan's rule. In this case, therefore, the problem of the comet is reduced to infinite series, or to the arithmetic of sines. If the given point is in the vertex of the curve, that is, in the perihelion, the problem is always resolvable, being reduced to the simple extraction of a cube root; and this is the case of comets which fall into the sun.

The resolvable case of the *lemniscata* is in the same circumstances, as may easily be seen by inspecting its equation.

In substituting for $\int y\, d\, x$, its value in our general equation, we may either give it in terms of x, that is, of the abscissa; or in terms of $x\, y$, that is, of the circumscribing rectangle; and neglect any further substitution. Thence arises a different and more elegant solution of the problem, by the intersection of curve lines; for we obtain an equation to a new curve, which cuts the former in the point required. Thus, by such a process in the case of the comet, we obtain the equation $y = \frac{6\, m\, \mathrm{D}^2}{(m+n)\,(x+3\,c)}$ to a conic hyperbola. For brevity's sake, put $\frac{2\, m\, \mathrm{D}^2}{m+n} = \phi^2$, the equation becomes $y = \frac{3\,\phi^2}{x+3\,c}$: Therefore, taking a point on the axis at the distance of $3\,c$ beyond the given vertex (or perihelion), erect a perpendicular, and between the two lines, as asymptotes, describe the hyperbola $y\, x = 3\,\phi^2$, it will cut the given trajectory in the point required: If the given point is in the perihelion, then the perpendicular must be raised at the vertex of the parabola.

The solution here given by a *locus*, is evidently general, and has no impossible case. But there are some instances in which such solutions, although perhaps the only practicable ones, are nevertheless attended with an impossible case. Let us take that of the *lemniscata*. Instead of the irresoluble equation of the sixth order, we obtain, by the last-mentioned method, a cubic equation of this form, $y = \frac{(3\,\phi^2 - 2\,a^2)\, x}{3\,c\, x - x^2 - 2\,a^2}$;

to a curve of the third order, called, if I rightly remember, by Sir Isaac Newton, in his "*Enumeratio Linearum Tertii Ordinis*," a *parabolism of the hyperbola*. Now, although this is extremely simple, in comparison of the complex equation given by the direct method first mentioned, it has manifestly one impossible case, viz., when ϕ is equal to $a \times \sqrt{\frac{2}{3}}$, or when the given area is to two-thirds of the square of the diameter of the curve, as $m + n$ to m: In this case, no parabolism of the hyperbola can be drawn, which will intersect the given curve in the point required; and this is an impossibility affecting every possible value of c; that is, every position of the given point, in this particular magnitude of the given area. But this circumstance makes no difference on the resolution of the problem by the direct method. Thus, when the eccentricity vanishes, or the given point is in the *punctum duplex*, the solution is derived from a cubic equation equally resolvable when $\phi = a \sqrt{\frac{2}{3}}$ as when ϕ is either $<$ or $> a \times \sqrt{\frac{2}{3}}$.

The method of resolving this interesting problem by *loci*, is the source of an immense variety of the most curious propositions concerning the properties and mutual relations of curve lines; and, more especially, leads us to the discovery of various porisms, which we otherwise should never have found out. In order to generalize and extend these, it is necessary that, instead of considering merely the case of Kepler's problem, where an area is cut by a straight line, we should consider also the far more difficult problem of cutting the area of one curve by another curve, in a given ratio; and then the problem may be extended to the section, not of one curvilinear area, but of an infinite number of areas, contained between two given lines, or of the areas of all the curves of a particular kind which can be drawn between those given lines. It is easy to perceive, that the same

resolution before adverted to, will not apply to those more complicated problems. But the reader will find a variety of examples of this species of proposition in the 'Philosophical Transactions of the Royal Society of London for 1798,' which were investigated chiefly in the manner above described.* It is evident that the application of such problems to physics does not proceed so far; for we have never yet discovered an example of a central force acting in a curvilinear direction.

The solutions now described, of Kepler's problem, and of several problems of a more general sort, are of a theoretical nature. They exhibit the mode of expressing by curve lines, or imaginary relations of known quantities, the relation required of the quantities given; they rather vary the difficulty, or simplify the relation, than remove the impediments to practical measurement. If it be required to exhibit the anomaly of the eccentric, we may indeed adopt the solution given by Sir Isaac Newton (*Principia*, lib. i. prop. 31, and Schol.), or that hinted at by Kepler himself. The Newtonian solution proceeds upon the description of a cycloid, and an easy construction, by which the point required is found in the intersection of a straight line with the given trajectory. In the tract referred to, a solution is given more directly, by the intersection of a species of a cycloid of easy description, with the given curves, without any subsequent construction. But these solutions, though more pleasing and beautiful in theory, are useless, when it is required to exhibit a value of the *abscissa* corresponding to the anomaly of the eccentric, or its supplement, in such a manner that a comparison may be made of this line with some known measure of length. It becomes necessary, in this case, to find a numerical value of the quantity in question. Now, this can only be done by a series; and the two great objects in finding such a series are, first, to give one which may be regulated by a simple law; and, secondly, to give one which may converge rapidly: so

* The Paper is given in this volume: it is the First Tract.

that its denominators rapidly increasing, the quantities may soon become so small, as not to deserve attention in our computations.

The approximation given by Mr. Ivory in his paper in the 'Edinburgh Transactions,'* deserves the first place among those of which we are in possession, whether we consider its simplicity, universality, or accuracy. The series is of easy management, applies to the most eccentric orbits, as well as to those approaching nearer to the circle, and to all degrees of eccentricity in the given point, the centre of forces. It has the benefit, too, of a most rapid convergence.

He first gives a very simple and elegant geometrical method of approximation, by an application of the rectangular case of the general problem *de inclinationibus* of the ancient geometers. But as this is by no means satisfactory to the practical calculator (for reasons before assigned), he proceeds next to the algebraic solution.

He begins with investigating the series for the eccentric anomaly when the mean anomaly is a right angle. It converges quickly, and the terms err alternately, by defect and excess, the difference growing continually less and less.

He then proceeds to the investigation of a similar series, found in the same manner, for the other cases of the mean anomaly. I should in vain attempt to give the reader a more minute idea of this solution, without a detail as full as the paper now before us, and shall only note an *erratum* that has crept into the twelfth article. After putting tan. A $= e \times \frac{\text{sin. } \phi}{\text{cos. } \frac{\phi - m}{2}} \times$ sec. $45°$, he infers that sin. $\frac{\psi}{2}$ = tan. $\frac{A}{2} \times 45°$; it should be sin. $\frac{\psi}{2}$ = tan. $\frac{A}{2} \times$ sin. $45°$.

He next gives two examples of the application of his method to geometric problems, concerning the circle. The one, is to bisect a given semicircular area by a chord from a

* Vol. v. p. 111. 1802.

given point in the circumference. The results of the series which he gives for the eccentric anomaly are as follows:—

Eccent. anom. = 47° 4′ (first value, and less than the truth).
,, = 47° 40′ 14″ (second value, and greater than the truth).
,, = 47° 39′ 12″ (third value, and less than the truth).

From this example, may be perceived the excellence of the method; for, whereas the first two terms differ by nearly 36′, the second and third differ only by 1′ 2″; or, in other words, while, by the two first trials, we come to a space of above half a degree, in some part of which the point required is to be found; by the second and third trials, we obtain a space of about the sixtieth part of a degree, in some part of which lies the result. By the third term of the series, then, we obtain a solution not more than 31″ distant from the truth, and this in circumstances the least favourable.

The other example is a solution of the problem—"to draw from a point in the circumference two chords which shall trisect the circular area." Here the

Eccent. anom. = 30° 33′ (first value less).
,, = 304° 4′ 11″ (second greater).

Euler's solution (*Analysis*, Inf. XI. 22) differs litte more than 30″ from this solution, given by Mr. Ivory's *second* term.

This specimen will sufficiently show the superior excellency of Mr. Ivory's method. Former analysts have only resolved the case within the eccentricity is small: his solution extends to comets as well as planets. For the planets, his rules apply with peculiar accuracy and ease; and his series converges with extreme rapidity; so much so, that we may consider the approximation of one term sufficient for practice. He has given a table of the values of the errors (or differences) for the different planets computed in this way. He adds an exemplification for the famous comet of 1682, supposed to be the same which reappeared in 1759. His first approximation

for the anomaly of the eccentric, reckoned from the aphelion (16 days 4 hours and 44′ from its perihelial passage), is 173° 51′, and too small. The second approximation is 173° 54′ 36″, exceeding the real eccentric anomaly from the perihelion by only a few seconds.

The application of the author's last correction, deduced from the comparison of the parabolic and elliptic trajectories, to the finding of the heliocentric place, and also the heliocentric distance (or *radius vector* of the cometic orbit), concludes this paper. I have been the more gratified by a perusal of this last branch of Mr. Ivory's inquiry, because the speculations had formerly occurred to me in a similar form. The introduction of the parabola, which admits of quadrature, and of definite solution, so far as regards Kepler's problem, has always appeared to me the surest method of rectifying the computations of the heliocentric places and distances of comets, or of their perihelial eccentric anomalies and *radii vectores*, during the small perihelial part of their trajectories which we are permitted to contemplate. In that part, the eccentric ellipse and the parabola nearly coincide; and, after all, we are not perfectly certain that those singular bodies do not move in orbits strictly parabolic.*

* This Paper appeared in the Second Number of the 'Edinburgh Review,' January, 1803.

III.

DYNAMICAL PRINCIPLE.—CALCULUS OF PARTIAL DIFFERENCES.—PROBLEM OF THREE BODIES.

THE pleasures of a purely scientific life have often been described; and they have been celebrated with very heartfelt envy by those whose vocations precluded or interrupted such enjoyments, as well as commended by those whose more fortunate lot gave them the experience of what they praised; but it may be doubted, if such representations can ever apply to any pursuits so justly as to the study of the mathematics. In other branches of science the student is dependent upon many circumstances over which he has little control. He must often rely on the reports of others for his facts; he must frequently commit to their agency much of his inquiries; his research may lead him to depend upon climate, or weather, or the qualities of matter, which he must take as he finds it; where all other things are auspicious, he may be without the means of making experiments, of placing nature in circumstances by which he would extort her secrets; add to all this the necessarily imperfect nature of inductive evidence, which always leaves it doubtful if one generalisation of facts shall not be afterwards superseded by another, as exceptions arise to the rule first discovered. But the geometrician relies entirely on himself; he is absolute master of his materials; his whole investigations are conducted at his own good pleasure, and under his own absolute and undivided control. He seeks the aid of no assistant, requires the use of no apparatus, hardly wants any books; and with the fullest reliance on the perfect instruments of his operations, and on the altogether certain nature of his results, he is quite assured that the truths which he has found out, though they

may lay the foundation of further discovery, can never by possibility be disproved, nor his reasonings upon them shaken, by all the progress that the science can make to the very end of time.

The life of the Geometrician, then, may well be supposed an uninterrupted calm; and the gratification which he derives from his researches is of a pure and also of a lively kind, whether he contemplates the truths discovered by others, with the demonstrative evidence on which they rest, or carries the science further, and himself adds to the number of the interesting truths before known. He may be often stopped in his researches by the difficulties that beset his path; he may be frustrated in his attempts to discover relations depending on complicated data which he cannot unravel or reconcile; but his study is wholly independent of accident; his reliance is on his own powers; doubt and contestation and uncertainty he never can know; a stranger to all controversy, above all mystery, he possesses his mind in unruffled peace; bound by no authority, regardless of all consequences as of all opposition, he is entire master of his conclusions as of his operations; and feels even perfectly indifferent to the acceptance or rejection of his doctrines, because he confidently looks forward to their universal and immediate admission the moment they are comprehended.

It is to be further borne in mind, that from the labours of the Geometrician are derived the most important assistance to the researches of other philosophers, and to the perfection of the most useful arts. This consideration resolves itself into two: one is the pleasure of contemplation, and consequently is an addition to the gratificatien of exactly the same kind, derived immediately from the contemplation of pure mathematical truth; much, indeed, of the mixed mathematics is also purely mathematical investigation, built upon premises derived from induction. The other gratification is of a wholly different description; it is connected merely with the promotion of arts subservient to the ordinary enjoyments of life. This is only a secondary and mixed use of science to the philosopher;

the main pleasure bestowed by it is the gratification which, by a law of our nature, we derive from contemplating scientific truth, when indulging in the general views which it gives, marking the unexpected relations of things seemingly unconnected, tracing the resemblance, perhaps identity, of things the most unlike, noting the diversity of those apparently similar. This is the true and primary object of scientific investigation. This it is which gives the pleasure of science to the mind. The secular benefits, so to speak, the practical uses derived from it, are wholly independent of this, and are only an incidental, adventitious, secondary advantage. (See Introductory Remarks to this volume.)

It is an illustration of the happiness derived from mathematical studies, that they possess two qualities in the highest degree, not perhaps unconnected with one another. They occupy the attention, entirely abstracting it from all other considerations; and they produce a calm agreeable temper of mind.

Their abstracting and absorbing power is very remarkable, and is known to all geometricians. Every one has found how much more swiftly time passes when spent in such investigations, than in any other occupation either of the senses or even of the mind. Sir Isaac Newton is related to have very frequently forgotten the season of meals, and left his food awaiting for hours his arrival from his study. A story is told of his being entirely shut up and disappearing, as it were eclipsed, and then shining forth grasping the great torch which he carried through the study of the heavens; he had invented the Fluxional Calculus. I know not if there be any foundation for the anecdote; but that he continually remained engaged with his researches through the night is certain, and that he then took no keep of time is undeniable. It does not require the same depth of understanding to experience the effects of such pursuits in producing complete abstraction; every geometrician is aware of them in his own case. The sun goes down unperceived, and the night wanes afterwards till he again rises upon our labours.

They who have experienced an incurable wound in some

prodigious mental affliction, have confessed, that nothing but mathematical researches could withdraw their attention from their situation. Instances are well known of a habit of drinking being cured by the like means; an inveterate taste for play has, within my own observation, been found to give way before the revival of an early love of analytical studies. This is possibly a cause of the other tendency which has been mentioned, the calming of the mind. Simson (the restorer of the Greek geometry) tells us how he would fly from the conflicts of metaphysical and theological science, to that of necessary truth, and how in those calm retreats he ever "found himself refreshed with rest." Greater tranquillity is possessed by none than by geometricians. Even under severe privations this is observed. The greatest of them all, certainly the greatest after Newton, was an example. Euler lost his sight after a long expectation of this calamity, which he bore with perfectly equal mind; both in the dreadful prospect and the actual bereavement, his temper continued as cheerful as before; his mind, fertile in resources of every kind, supplied the want of sight by ingenious mechanical devices, and by a memory more powerful even than before.* He furnishes an

* My late learned and esteemed friend, Mr. Gough, of Kendal, was another example of studies being pursued under the same severe deprivation—but he had never known the advantages of sight, having lost his eyes when an infant, and never had any distinct recollection of light. He was an accomplished mathematician of the old school, and what is more singular, a most skilful botanist. His prodigious memory resembled Euler's, and the exquisite acuteness of his smell and touch supplied in a great measure the want of sight. He would describe surfaces as covered with undulations which to others appeared smooth and even polished. His ready sagacity in naming any plant submitted to his examination was truly wonderful. I had not only the pleasure of his acquaintance, but I have many particulars respecting his rare endowments, from another eminent mathematician, who unites the learning of the older with that of the modern school, my learned friend and neighbour, Mr. Slee, of Tirrel. A detailed account of Mr. Gough's case, by Mr. Slee and Professor Whewell (a pupil of his), would be most curious and instructive. Euler's memory was such, that he could repeat the Æneid, noting the words that begin and end each page. Mr. Gough also was an excellent classical scholar.

instance to another purpose. Thoughtless and superficial observers have charged this science with a tendency to render the feelings obtuse. Any pursuit of a very engrossing or absorbing kind may produce this temporary effect; and it has been supposed that men occasionally abstracted from other contemplations, are particularly dull of temper. But no one ever had more warm or kindly feelings than Euler, whose chief delight was in the cheerful society of his grandchildren, to his last hour; and whose chief relaxation from his severer studies was found in teaching these little ones.

It has been alleged, and certainly has been somewhat found by experience to be true, that the habit of contemplating necessary truth, and the familiarity with the demonstrative evidence on which it rests, has a tendency to unfit the mind for accurately weighing the inferior kind of proof which we can alone obtain in the other sciences. Once finding that the certainty to which the geometrician is accustomed cannot be attained, he is apt either to reject all testimony, or to become credulous by confounding different degrees of evidence, regarding them all as nearly equal from their immeasurable inferiority to his own species of proof—much as great sovereigns confound together various ranks of common persons, on whom they look down as all belonging to a different species from their own. In this observation there is, no doubt, much of truth; but we must be careful not to extend its scope too far, so as that it should admit of no exceptions. D'Alembert affords one of the most remarkable of these; as far as physical science went, Laplace afforded another; in several other branches he was, perhaps, no exception to the rule.

Whatever of peace and comfort he enjoyed, D'Alembert owed to geometry, and confessed his obligations. Whatever he suffered from vexation of any sort, he could fairly charge upon the temporary interruption of his mathematical pursuits. Both portions of his history, therefore, enforce the doctrine which I have laid down.

His 'Traité de Dynamique' at once placed him in the

highest rank of geometricians. The theory is deduced with perfect precision, and with as great clearness and simplicity as the subject allows, from a principle which he first laid down and explained, though it be deducible from the equality of action and re-action, a physical rather than a mathematical truth, and derived from universal induction, not from abstract reasoning à priori.

The Principle is this ('Dyn.' part 2, chap. i.). If there are several bodies acting on each other, as by being connected through inflexible rods, or by mutual attraction, or in any other way that may be conceived; suppose an external force is impressed upon those bodies, they will move not in the direction of that force as they would were they all unconnected and free, but in another direction; then the force acting on the bodies may be decomposed into two, one acting in the direction which they actually take, or moving the bodies without at all interfering with their mutual action, the other in such direction as that the forces destroy each other and are wholly extinguished; being such, that if none other had been impressed upon the system, it would have remained at rest.* This principle reduces all the problems of dynamics to statical problems, and is of great fertility, as well as of admirable service in both assisting our investigations and simplifying them. It is, indeed, deducible from the simplest principles, and especially from the equality of action and re-action; but though any one might naturally enough have thus hit upon it, how vast a distance lies between the mere principle and its application to such problems, for example, as to find the locus or velocity of a body sliding or moving

* Lagrange's statement of the principle is the most concise, but I question if it is the clearest, of all that have been given. "If there be impressed upon several bodies, motions which they are compelled to change by their mutual actions, we may regard these motions as composed of the motions which the bodies will actually have, and of other motions which are destroyed; from whence it follows, that the bodies, if animated by those motions only, must be in equilibrio." ('Méc. An.' vol. i. p. 239, Ed. 1811.) It is not easy to give a general statement of the principle, and I am by no means wedded to the one given in the text. (See note III.)

freely along a revolving rod, at the extremity of which rod a fixed body moves round in a given plane—a locus which the calculus founded on the Principle shows to be in certain cases the logarithmic spiral.*

No one can doubt that the Principle of D'Alembert was involved in many of the solutions of dynamical problems before given. But then each solution rested on its own grounds, and these varied with the different cases; their demonstrations were not traced to and connected with one fundamental principle. He alone and first established this connection, and extended the Principle over the whole field of dynamical inquiry.

The 'Traité' contains, further (part 1, chap. ii.), a new demonstration of the parallelogram of forces. The reason of the author's preference of this over the common demonstration is not at all satisfactory. His proof consists in supposing the body to move on a plane sliding in two grooves parallel to one side of the parallelogram, and at the same time carried along in the direction of the other side. This is not one whit more strict and rigorous than the ordinary supposition of the body moving along a ruler parallel to one side, while the ruler at the same time moves along a line parallel to the other side. Indeed I should rather prefer this demonstration to D'Alembert's.

The 'Traité de Dynamique' appeared in 1743; and in the following year its fundamental principle was applied by the author to the important and difficult subject of the equilibrium and motion of fluids, the portion of the 'Principia' which its illustrious author had left in the least perfect state. Pressed by the difficulty of the inquiry, which is one of the most important in Hydrodynamics, the motion of a fluid through an orifice in a given vessel, and despairing of the data afford-

* The general equation is $d^2y = \frac{y\,dx^2}{a^2} + \frac{2\,\mathrm{D}\,y\,dy^2}{\mathrm{A}\,a^2 + \mathrm{D}\,y^2}$ in which y is the distance of the moving body D from the fixed point, or the length of the rod, at the end of which is the body A, describing an arch of a circle, and x that arch. The velocity of D is likewise found in terms of the same quantity.

ing the means of a strict and direct solution, Newton had recourse to assumptions marked by the most refined ingenuity, but admitted to be gratuitous and to be unauthorised by the facts. The celebrated Cataract is of this description. He supposes ('Principia,' lib. ii. prop. 36), that a body of ice shaped like the vessel, comes in contact with the upper surface of the liquid and melts immediately on touching it, so as to keep the level of the fluid always the same, and that a cataract is thus formed, of which the upper surface is that of the fluid, and the lower that of the orifice. His first investigation assumed the issuing column to be cylindrical, but he afterwards found that the lateral pressure and motion gave it the form of a truncated cone which he called a vein; and his correction of the former result was a matter of much controversy among mathematicians. Daniel Bernoulli at first maintained it to be erroneous against Riccati and others; but he afterwards acquiesced in Newton's view. He, however, always resisted the hypothesis of the cataract, as indeed did most other inquirers. Newton's assumptions, in other parts of this very difficult inquiry, have been deemed liable to the same objections; as where he leaves the purely speculative hypothesis of perfectly uncompressed and distinct particles, and treats of the interior and minute portions of fluids, as similar to those which we know. (Lib. ii. prop. 37, 38, 39.) It must, however, be admitted, as D'Alembert has observed ('Encyc.' v. 889, and 'Résistance des Fluides,' xvii.) that "those who attacked the Newtonian theory on this subject had no greater success than its illustrious author; some having, after resorting to hypotheses which the experiments refuted, abandoned their doctrines as equally unsatisfactory, and others confessing their systems groundless, and substituting calculations for principles."

Such was the state of the science when D'Alembert happily applied his Dynamical principle to the pressure and motion of fluids, and found that it served excellently for a guide, both in regard to non-elastic and elastic fluids. In fact, the particles of these being related to one another by a cohesion

which prevents them not from obeying an external impulse, it is manifest that the principle may be applied. Thus, if a fluid contained in a vessel of any shape be conceived divided into layers perpendicular to the direction of its motion, and if v represent generally the velocity of the layers of fluid at any instant, and dv the small increment of that velocity, which may be either positive or negative, and will be different for the different layers, $v \pm dv$ will express the velocity of each layer as it takes the place of that immediately below it; then if a velocity $\mp dv$ alone were communicated to each layer, the fluid would remain at rest. ('Traité de Fluides,' liv. ii. chap. 1, theor. 2). Thus the velocity of each part of the layer being taken in the vertical direction is the same, and this velocity being that of the whole layer itself, must be inversely as its horizontal section, in order that its motion may not interfere with that of the other layers, and may not disturb the equilibrium. This, then, is precisely the general dynamical principle already explained applied to the motion of fluids, and it is impossible to deny that the author is thus enabled to demonstrate directly many propositions which had never before been satisfactorily investigated. It is equally undeniable that much remained after all his efforts incapable of a complete solution, partly owing to the inherent difficulties of the subject from our ignorance of the internal structure and motions of fluids, and partly owing to the imperfect state in which all our progress in analytical science still has left us, the differential equations to which our inquiries lead having, in very many cases, been found to resist all the resources of the integral calculus.

This remark applies with still greater force to his next work. In 1752, he published his Essay on a new theory of the Resistance of Fluids. The great merit of this admirable work is that it makes no assumption, save one to which none can object, because it is involved in every view which can well be taken of the nature of a fluid; namely, that it is a body composed of very minute particles, separate from each other, and capable of free motions in all directions. He

applies the general dynamical principle to the consideration of resistance in all its views and relations, and he applies the calculus to the solution of the various problems with infinite skill. It is in this work that he makes the most use of that refinement in the integral calculus of which we shall presently have occasion to speak more at large, as having first been applied by D'Alembert to physical investigation, if it was not his own invention. But the interval between 1744 and 1752 was not passed without other important contributions to physical and analytical science. In 1746, he gave his Memoir on the general theory of Winds, which was crowned by the Royal Academy of Berlin. The foundation of this able and interesting inquiry is the influence of the sun and moon upon the atmosphere, the aërial tides, as it were, which the gravitation towards these bodies produces; for he dismisses all other causes of aërial currents as too little depending upon any definite operation, or too much depending upon various circumstances that furnish no precise data, to be capable of analytical investigation. The Memoir consists of three parts. In the *first* he calculates the oscillations caused by the two heavenly bodies supposing them at rest, or the earth at rest in respect of them. In the *second*, he investigates their operation on the supposition of their motion. In the *third*, he endeavours to trace the effects produced upon the oscillations by terrestrial objects. The paper is closed with ramarks upon the effects of temperature. The whole inquiry is conducted with reference to the general dynamical principle which he had so happily applied to the equilibrium and pressure of fluids, in his first work upon that difficult subject.

In treating of Hydrodynamics, D'Alembert had found the ordinary calculus insufficient, and was under the necessity of making an important addition to its processes and its powers, already so much extended by the great improvements which Euler had introduced. This was rendered still more necessary when, in 1746, he came to treat of the winds, and in the following year when he handled the very difficult subject of the vibration of cords, hitherto most imperfectly investigated

by mathematicians.* In all these inquiries the differential equations which resulted from a geometrical examination of the conditions of any problem, proved to be of so difficult integration that they appeared to set at defiance the utmost resources of the calculus. When a close and rigorous inspection showed no daylight, when experiments of substitution and transformation failed, the only resource which seemed to remain was finding factors which might, by multiplying each side of the equation, complete the differential, and so make it integrable either entirely, or by circular arches, or by logarithms, or by series. D'Alembert, in all probability, drew his new method of treating the subject from the consideration that, in the process of differentiation we successively assume one quantity only to be variable and the rest constant, and we differentiate with reference to that one variable; so that $x\,dy + y\,dx$ is the differential of xy, a rectangle, and $xy\,dz + xz\,dy + yz\,dx$ the differential of xyz, a parallelopiped, and so of second differences, d^2z being (when $z = x^m$) $= (m^2 - m)\,x^{m-2}\,dx^2 + m\,x^{m-1}\,d^2x$. He probably conceived from hence that by reversing the operation and partially integrating, that is, integrating as if one only of the variables were such, and the others were constant, he might succeed in going a certain length, and then discover the residue by supposing an unknown function of the variable which had been assumed constant, to be added, and after-

* Taylor ('Methodus Incrementorum') had solved the problem of the vibrating cord's movement, but upon three assumptions—that it departs very little from the axis or from a straight line, that all its points come to the axis at the same moment, and that it is of a uniform thickness in its whole length. D'Alembert's solution only requires the last and the first supposition, rejecting the second. The first, indeed, is near the truth, and it is absolutely necessary to render the problem soluble at all. The third has been rejected by both Euler and Daniel Bernoulli, in several cases investigated by them. D'Alembert's solution led to an equation of partial differences of this form $\left(\frac{d^2y}{dt^2}\right) = a^2\left(\frac{d^2y}{dx^2}\right)$ in which t is the time of the vibration, x and y the co-ordinates of the curve formed by the vibration.

wards ascertaining that function by attending to the other conditions of the question. This method is called that of *partial differences*. Lacroix justly observes that it would be more correct to say *partial differentials;* and a necessary part of it consisted of the *equations of conditions*, which other geometricians unfolded more fully than the inventor of the calculus himself; that is to say, statements of the relation which must subsist between the variables or rather the differentials of these variables, in order that there may be a possibility of finding the integral by the method of partial differences. It appears that Fontaine, a geometrician of the greatest genius, gave the earliest intimation on this important subject; for the function of one or both variables which is multiplied by $d\,x$ being called M, and that function of one or both which is multiplied by $d\,y$ being called N, the canon or criterion of integrability is that

$$\frac{d\,\mathrm{M}}{d\,y} = \frac{d\,\mathrm{N}}{d\,x};$$

and we certainly find this clearly given in a paper of Fontaine's read before the Academy, 19th November, 1738. It is the third theorem of that paper. Clairaut laid down the same rule in a Memoir which he presented in 1739; but he admits in that Memoir his having seen Fontaine's paper. He expounds the subject more largely in his far fuller and far abler paper of 1740; and there he says that Fontaine showed his theorem to the Academy the day this second paper of Clairaut's was read—erroneously, for Fontaine had shown it in November, 1738; and had said that it was then new at Paris, and was sent from thence to Euler and Bernoulli. The probability is, that Clairaut had discovered it independent of Fontaine, as Euler certainly had done; and both of them handled it much more successfully than Fontaine. D'Alembert, in his demonstrations, 1769, of the theorems on the integral calculus, given by him without any demonstration in the volume for 1767, and in the scholium to the twenty-first theorem, affirms distinctly that he had communicated to Clairaut a portion of the demonstration, forming a corollary

to the proposition, and from which he says that Clairaut derived his equation of condition to differentials involving three variables. It is possible; but as this never was mentioned in Clairaut's lifetime, although there existed a sharp controversy between these two great men on other matters, and especially as the equation of conditions respecting two variables might very easily have led to the train of reasoning by which this extension of the criterion was found out, the probability is, that Clairaut's discovery was in all respects his own.

The extreme importance of this criterion to the method of partial differences, only invented, or at least applied, some years later, is obvious. Take a simple case in a differential equation of the first order,—

$$dz = (2axy - y^3)\,dx + (ax^2 - 3xy^2)\,dy,$$

where $\quad M = 2axy - y^3, \quad N = ax^2 - 3xy^2.$

For the criterion $\quad \dfrac{dM}{dy} = 2ax - 3y^2$

$$\frac{dN}{dx} = 2ax - 3y^2$$

gives us $\quad \dfrac{dM}{dy} = \dfrac{dN}{dx},$

which shows that the equation $M\,dx + N\,dy$ is a complete differential, and may be integrated. Thus integrate $(ax^2 - 3xy^2)\,dy$, as if x were constant, and add X (a function of x, or a constant), as necessary to complete the integral, and we have $\quad ax^2y - xy^3 + X = Z;$

now differentiate, supposing y constant, and we have

$$\frac{dz}{dx} = (2axy - y^3) + \frac{dX}{dx}$$

(because of the criterion) $= 2axy - y^3,$

consequently $\dfrac{dX}{dx} = o$, and $X = C$, a constant.

Accordingly, $\quad z = ax^2y - xy^3 + C;$

and so it is, for differentiating in the ordinary way, x and y being both variable, we have

$$\begin{aligned} dz &= 2axy\,dx + ax^2\,dy - 3xy^2\,dy - y^3\,dx \\ &= (2axy - y^3)\,dx + (ax^2 - 3xy^2)\,dy; \end{aligned}$$

which was the equation given to be integrated.

To take another instance in which $\frac{dX}{dx}$, the differential coefficient of the quantity added is not $= o$ or X constant. Let

$$dz = y^2\,dx + 3x^2\,dx + 2xy\,dy,$$

in which, by inspection, the solution is easy—

$$z = xy^2 + x^3 + C.$$

Here $$M = y^2 + 3x^2, \qquad N = 2xy,$$

and $$\frac{dM}{dy} = 2y = \frac{dN}{dx}.$$

So $z = xy^2 + X$, and differentiating with respect to x,

$$\frac{dz}{dx} = y^2 + \frac{dX}{dx} = y^2 + 3x^2.$$

Hence $$X = x^3 + C,$$

and $$z = xy^2 + x^3 + C,$$

the integral of the equation proposed.

It must, however, be observed of the criterion, that an equation may be integrable which does not answer the condition

$$\frac{dM}{dy} = \frac{dN}{dx}.$$

It may be possible to separate the variables and obtain $X\,dx = Y\,dy$, as by transformation; or to find a factor, which, multiplying the equation, shall render it integrable, by bringing it within that condition. The latter process is the most hopeful; and it is generally affirmed that such a factor, F, may always be found for every equation of the first order involving only two variables. However, this is only true in theory: we cannot resolve the general equation by any such means; for that gives us

$$F.\left(\frac{dM}{dy} - \frac{dN}{dx}\right) = N.\frac{dF}{dx} - M\frac{dF}{dy},$$

an expression as impossible to disentangle, it may safely be asserted, as any for the resolution of which its aid might be wanted. It is only in a few instances of the values of these functions (M and N) that we can succeed in finding F. It is quite unaccountable * that Clairaut should, in reference to his equation, which is substantially the same with the above, describe it as "d'une grande utilité, pour trouver μ" (that is F).

It is here to be observed, that not only Fontaine had, apparently, first of all the geometricians, given the criterion of integrability, but he had also given the notation which was afterwards adopted for the calculus of Partial Differences. ϕ being a function of two variables, x and y, he makes $\frac{d\phi}{dx}$ stand for the differential coefficient of ϕ when x only varies, and $\frac{d\phi}{dy}$ for the same differential coefficient when y only varies. Hence he takes $\frac{d\phi}{dx} \times dx$, not, as in the ordinary notation it would be, $= d\phi$, the complete differential of ϕ; whereas that differential would, in this solution, be

$$\frac{d\varphi}{dx} \times dx + \frac{d\phi}{dy} \times dy.$$

Thus, if $\phi = xy^2$, its complete dif. $d\phi = 2yx\,dy + y^2\,dx$, but

$$\frac{d\phi}{dx} = y^2.$$

It is quite clear, therefore, that Fontaine gave the notation of this calculus.

But D'Alembert had been anticipated in the method itself, as well as in the notation or algorithm; for Euler, in a paper entitled 'Investigatio functionum ex datâ differentialium conditione,' dated 1734,† integrated an equation of partial differ-

* Mem. de l'Acad. 1740, p. 299.—I find my surprise shared by a very learned mathematician to whom I had mentioned it, Prof. Heaviside.

† 'Petersburgh Memoirs,' vol. vii.—That Euler, in the Memoir published in 1734, solved an equation of Partial Differences is quite incon-

ences; and he had afterwards forgotten his own new calculus, so entirely as to believe that it was first applied by D'Alembert in 1744. So great were the intellectual riches of the first of analysts, that he could thus afford to throw away the invention of a new and most powerful calculus! A germ of the same method is plainly to be traced in Nicolas Bernoulli's paper* in the 'Acta Eruditorum' for 1720, on Orthogonal Trajectories.

While mentioning Fontaine's great and original genius for analytical investigations, we must not overlook his having apparently come very near the Calculus of Variations. In a

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testable, though he laid down no general method; which, indeed, D'Alembert himself never did, nor any geometrician before the publication of Euler's third vol. of the 'Institutions of the Integral and Differential Calculus.' The problem, as given in the 'Mem. Acad. Petersb.' vol. vii. was this: We have the equation $dz = P\,dx + Q\,da$, z being a function of x and a; and the problem is to find the most general value of P and Q, which will satisfy the equation. $Q = Fz + PR$, F being a function of a, and R a function of a and x, Euler seeks for the factor which will make $dx + R\,da$ integrable. Call this factor S, and make $S\,dx + S\,R\,da = dT$, and make $\int F\,da = \log. B$.

He finds for the values required

$$P = BSf' : T,\ Q = \frac{z\,dB}{B\,da} + BRSf : T;$$

and from thence he deduces

$$dz = BS\,(dx + R\,da)\,f' : T + z\frac{dB}{B} = B\,df : T + z\frac{dB}{B},\ \text{and}$$

consequently $z = Bf : T$.

It is thus clear, that Euler had, in or before 1734, integrated an equation of Partial Differences; and it must further be remarked, that D'Alembert, in his paper on the Winds, the first application of the calculus, quotes Euler's paper of 1734. D'Alembert always differed with Euler respecting the extent to which this calculus can be applied, holding, contrary to Euler's opinion, that it does not include irregular and discontinuous arbitrary functions.[1]

* See, too, the paper in John Bernoulli's Works, vol. ii. p. 442, where he investigates the transformation of the differential equation $dx = P\,dy$ (P being a function of a, x, and y) into one, in which a also is variable.

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[1] Cousin has mentioned the anticipation of Euler. 'Astronomie, Disc. Prélim.'

paper read at the Academy, 17th February, 1734, we find a passage that certainly looks towards that calculus, and shows that he used a new algorithm as requisite for conducting his operation:—"J'ai été obligé," he says, "de faire varier les mêmes lignes en deux manières differentes. Il a fallu designer leurs variations differemment." "J'ai marqué les unes commes les géomètres Anglais par des fluxions (points); les autres par des differences ($d\,x$) à nôtre manière; de sorte qu'ici $d\,x$ ne sera pas la même chose que $x, d\,x$ que x" (p. 18). "Il peut y avoir," he afterwards adds, "des problèmes qui dependroient de cette méthode fluxio-differentielle."

Nothing that has now been said can, in any manner, detract from the renown justly acquired by D'Alembert and Lagrange as the first who fully expounded the two great additions to the Differential Calculus—first applied them systematically to the investigation of physical as well as mathematical questions, and therefore may truly be said to have first taught the use of them as instruments of research to geometricians.*

In the year 1746 the Academy of France proposed, as the subject of its annual prize essay for 1748, the disturbances produced by Jupiter and Saturn mutually on each other's orbits. Euler's Memoir gained the prize; and it contains the solution of the famous Problem of the Three Bodies—namely, to find the path which one of those bodies describes round another when all three attract each other with forces varying inversely as the squares of their distances, their velocities and masses being given, and their directions in the tangents of their orbits.† This, which applies to the case of the Moon, would be resolved were we in possession of the solution for the case of Jupiter and Saturn, which, instead of

* There was nothing in the observation of Fontaine that can be termed an anticipation of Lagrange, though D'Alembert, unknown to himself, had certainly been anticipated by Euler.

† The problem of the Three Bodies, properly speaking, is more general; but, in common parlance, it is confined to the particular case of gravitation, and indeed of the sun, earth, and moon, as three bodies attracting each other by the law of gravitation, and one of which is incomparably larger than the other two.

revolving round each other, revolve round the third body. Euler's investigation did not appear quite satisfactory; and in 1750 the same subject was announced for 1752, when he again carried off the prize by a paper exhausting the subject, and affording such an approximation to the solution as the utmost resources of the integral calculus can give. But while we admit, because its illustrious author himself admitted, the justice of the Academy's views respecting his first solution, we must never forget the extraordinary genius displayed in it. He did not communicate the whole, or even the more essential portion of his investigation; but he afterwards gave it in a paper to the Berlin Academy in 1747, and in another to the Petersburgh Academy in 1750, the first of these containing our earliest view of the variation of arbitrary constants in differential equations, and the development of the radical which expresses the relative disturbance between two planets in a series of sines and cosines of angles multiples of the elongation, a series so artistly framed that every three consecutive terms are related together in such a manner as to give the whole series from a determination of the first two terms. Clairaut appears to have turned his attention to the same problem some time before Euler. In 1743, he gave a Memoir on the Moon's Orbit, according to the Newtonian theory of gravitation, and it appears in the volume for that year; but this paper must be admitted to have been a somewhat slight performance for so consummate a geometrician. It rather evaded the difficulties of the problem than surmounted by encountering them; for he assumed the orbit of the moon to differ imperceptibly from a circle; and his differential equation could not have been integrated without this supposition. Now, the only assumptions which had been conceived permissible were the incomparably greater mass of one body than those of the two others,* the nearly equal

* In truth, the mass of the sun being 355,000 times that of the earth, and that of the earth being between sixty-eight and sixty-nine times that of the moon, the mass of the sun is twenty-five millions of times greater than that of the moon.

distance of that body from each of the two others, and the almost elliptical path of the one whose orbit was sought, leaving its deviation from that path alone to be sought after. Accordingly, the paper of 1743 did not satisfy its illustrious author, who, in 1747, produced another worthy of the subject and of himself. This was read 15th November, 1747, but part of it had been read in August. He asserts positively in a note (Mém.' 1745, p. 335), that though Euler's first paper had been sent in the same year, he had never seen it till after his solution was obtained; therefore, Lalande had no right to state in his note to the very bad edition of Montucla which he published, that Fontaine always said that Clairaut was enabled to obtain his solution by the paper of Euler (vol. iv. p. 66).

At the time that Clairaut was engaged in this investigation, D'Alembert, unknown to him, was working upon the same subject. Their papers were presented on the same day, and Clairaut's solution was unknown to D'Alembert; but so neither could D'Alembert's solution have been known to Clairaut, because the paper is general on the problem, and the section applicable to the moon's orbit was added after the rest was first read, and was never read at all to the Academy. Nothing, therefore, can be more clear than that neither of these great geometricians borrowed from the other, or from Euler. It is just possible that Euler in his complete solution of 1752 might have had the advantage of their previous ones; but as it clearly flowed from his earlier paper, there is no doubt also of his entire originality. Nevertheless, when D'Alembert's name became mixed up with the party proceedings among the literary and fashionable circles of Paris, there were not wanting those who insisted that the whole fame of this great inquiry belonged to Clairaut; and it is painful to reflect on the needless uneasiness which such insinuations gave to D'Alembert.

Thus, in investigating this famous "Problem of the Three Bodies," all the three geometricians, without communicating

together, took the same general course in the field, like three navigators of consummate skill and most practised experience tracing the pathless ocean, unseen by one another, and each trusting to his seamanship, his astronomical observations, and his time-keeper, and all of them steering separately the same course. They were each led to three equations, which nearly resembled those obtained by the other two. Of the three equations the most important is—

$$\frac{d^2 u}{d v^2} + u + \frac{T \frac{d u}{d v} - P u}{u^3 \left(h^2 + 2 \int \frac{T}{u^3} d v \right)} = o,$$

in which u is the reciprocal of the projection on the plane of the ecliptic of the moon's distance from the earth, v the moon's longitude with respect to the centre of gravity of the earth and moon, P and T the resultants respectively of all the forces acting on the moon parallel and perpendicular to $\frac{1}{u}$, and parallel to the plane of the ecliptic, h an arbitrary constant. P and T being complicated functions of the longitudes of the sun and moon, as well as of the eccentricities of their orbits have to be developed for the further solution of the problem.

Now, it is a truly remarkable circumstance that the conclusion at which all these great men separately arrived was afterwards found to be erroneous. They made the revolving motion of the moon's apogee (or the revolution which the most distant part of her orbit makes in a certain time) half as much as the observations show it to be; and in a revolution of the moon, 1° 30′ 43″, instead of 3° 2′ 32″ the observations giving about nine years for the period, which the revolution really takes, instead of eighteen. Clairaut first stated this apparent failure of the Newtonian theory, and as he had taken pains to make the investigation "avec toute l'exactitude qu'elle demandoit"

('Mém.' 1745, p. 336), he was with great reluctance driven to conclude that the doctrine of gravitation failed to account for the progression of the apogee or revolution of the lunar orbit; and if so, as Euler justly observed (Prix., tom. vii., 'Recherches sur Jupiter et Saturne,' p. 4), we must have been entitled to call in question the operation of the same principle on all the other parts of the planetary system. Clairaut even went so far as to propose, in consequence of the supposed error, a modification of the law of gravitation; and that we should, instead of considering it as in the proportion of $\frac{1}{d^2}$, (d being the distance,) regard it as proportional partly to $\frac{1}{d^2}$, the inverse square, and partly to $\frac{1}{d^4}$, the inverse fourth power of the distance. But this suggestion was far from giving satisfaction even to those who admitted the failure of the theory. A controversy arose between this great geometrician and a very unworthy antagonist, Buffon, who on vague, metaphysical, and even declamatory grounds, persisted in showing his ignorance of analysis, and his obstinate vanity; nor, though he was by accident quite right, could any one give him the least credit for his good fortune. Clairaut answered him, and afterwards rejoined to his reply, with a courtesy which betokened entire civility and even respect for the person, with an infinitely low estimation of either his weight or his strength—quantities truly evanescent. At length it occurred to him that the process should be repeated, a course which he certainly must have taken at first had he not naturally enough been misled by the singular coincidence of both Euler and D'Alembert * having arrived at the same conclusion with himself. He found that he ought to have repeated his investigation of the differential equation to the radius, after obtaining, by a first investiga-

* Euler had stated it incidentally, as regarded the lunar apogee, in his prize memoir, in 1746, on Jupiter and Saturn, but he mentioned it more fully in a letter to Clairaut. ('Mém.' 1745, p. 353, note.)

tion, the value of the third term above given in that equation—

$$\frac{T\frac{du}{dv} - \&c.}{u^3(h^2 + \&c.}$$ (as above given.)

This omission he now supplied, and he found that the result, when applied to the case, made the progression of the moon's apogee twice as quick as the former operation had given it, or nine years, agreeing with the actual observation. He deposited, in July, 1746, with the secretary of the Academy, as well as with Sir Martin Folkes, president of the Royal Society, a sealed paper containing the heads of his analysis, but delayed the publication of it until he should complete the whole to his satisfaction: a most praiseworthy caution, after the error that had been committed in the first instance. He announced, however, the result, and its confirming the Newtonian theory, in May of the same year; and added, that his reasoning was purely geometrical, and had no reference to vague topics, giving, at the same time, a conclusive exposition of Buffon's ignorance in his hot attack, which showed him to be wholly incapable of appreciating any part of the argument. In May, 1752, the Memoir itself was given to the Academy, and it appears in the volume for 1748. It is entitled, "De l'Orbite de la Lune, en ne négligeant pas les quarrés des quantités de même ordre avec les forces perturbatrices;" which has misled many in their conception of the cause to which the error must be ascribed. But in the volume for 1748, p. 433, he leaves no doubt on that cause; for he states that having originally taken the radius vector r, (the reciprocal of u in our former equation,) $= \frac{k}{1 - \cos. mv}$, he now takes fully that reciprocal u or $\frac{k}{r} = 1 - e\cos. mv + \beta\cos.\frac{2v}{n} - \gamma\cos.\left(\frac{2}{n} - m\right)v + \delta\cos.\left(\frac{2}{n} + m\right)v - \zeta\cos.$

$\left(\frac{2}{n} - 2m\right)v$, terms obtained by the first or trial integration, which he had fully explained in his first Memoir to be the more correct mode of proceeding ('Mém.' 1745, p. 352); and the consequence of this is to give the multiplier, on which depends the progression of the apogee, a different value from what it was found to have in the former process. It is never to be forgotten that the original investigation was accurate as far as it went; but by further extending the approximation a more correct value of m was obtained, in consequence of which the expression for the motion of the apogee became double that which had been calculated before.

It should be observed, in closing the subject of the Problem of Three Bodies, that Euler no sooner heard of Clairaut's final discovery, than he confirmed it by his own investigation of the subject, as did D'Alembert. But in the meantime, Matthew Stewart had undertaken to assail this question by the mere help of the ancient geometry, and had marvellously succeeded in reconciling the Newtonian theory with observation. Father Walmisley, a young English priest of the Benedictine order, also gave an analytical solution of the difficulty in 1749.

The other great problem, the investigation of which occupied D'Alembert, was the Precession of the equinoxes and the Nutation of the earth's axis, according to the theory of gravitation. Sir Isaac Newton, in the xxxix. prop. of the third book, had given an indirect solution of the Problem concerning the Precession; the Nutation had only been by his unrivalled sagacity conjectured à priori, and was proved by the observations of Bradley. The solution of the Precession had not proved satisfactory; and objections were taken to the hypotheses on which it rested, that the accumulation of matter at the equator might be regarded as a belt of moons, that its movement might be reckoned in the proportion of its mass to that of the earth, and that the proportion of the terrestrial axes is that of 229 to 230; that the earth is homogeneous, and that the action of the sun and moon *ad mare movendum*, are as

one to four and a half nearly, and in the same ratio *ad equinoctia movenda.* Certainly the three last suppositions have since Newton's time been displaced by more accurate observations; the axes being found, to be as 298 to 299, the earth not homogeneous, and the actions of the sun and moon on the tides more nearly as one to three. But it has often been observed, and truly observed, that when D'Alembert came to discuss the subject, it would have been more becoming in him to assign his reasons for denying the other hypothesis on which the Newtonian investigation rests, than simply to have pronounced it groundless. However, it is certain that he first gave a direct and satisfactory solution of this great problem; and that he investigated the Nutation with perfect success, showing it to be such that if it subsisted alone (*i.e.*, if there were no precessional motion) the pole of the equinoctial would describe among the stars a minute ellipse, having its longer axis about 18″ and its shorter about 13″, the longer being directed towards the pole of the ecliptic, and the shorter of course at right angles to it. He also discovered in his investigations that the Precession is itself subject to a variation, being in a revolution of the nodes, sometimes accelerated, sometimes retarded, according to a law which he discovered, giving the equation of correction. It was in 1749 that he gave this admirable investigation; and in 1755 he followed it up with another first attempted by him, namely, the variation which might occur to the former results, if the earth, instead of being a sphere oblate at the poles, were an elliptic spheroid, whose axes were different. He added an investigation of the Precession on the supposition of the form being any other curve approaching the circle. This is an investigation of as great difficulty perhaps as ever engaged the attention of analysts. It remains to add that Euler, in 1750, entered on the same inquiries concerning Precession and Nutation; and with his wonted candour, he declared that he had read D'Alembert's memoir before he began the investigation.*

* This Tract is from 'Lives of the Philosophers'—Life of D'Alembert.

IV.

GREEK GEOMETRY.—ANCIENT ANALYSIS.—PORISMS.

The wonderful progress that has been made in the pure mathematics since the application of algebra to geometry, begun by Vieta in the sixteenth, completed by Des Cartes in the seventeenth century, and especially the still more marvellous extension of analytical science by Newton and his followers, since the invention of the Calculus, has, for the last hundred years and more, cast into the shade the methods of investigation which preceded those now in such general use, and so well adapted to afford facilities unknown while mathematicians only possessed a less perfect instrument of investigation. It is nevertheless to be observed that the older method possessed qualities of extraordinary value. It enabled us to investigate some kinds of propositions to which algebraic reasoning is little applicable; it always had an elegance peculiarly its own; it exhibited at each step the course which the reasoning followed, instead of concealing that course till the result came out; it exercised the faculties more severely, because it was less mechanical than the operations of the analyst. That it afforded evidence of a higher character, more rigorous in its nature than that on which algebraic reasoning rests, cannot with any correctness be affirmed; both are equally strict: indeed, if each be mathematical in its nature, and consist of a series of identical propositions arising one out of another, neither can be less perfect than the other, for of certainty there can be no degrees. Nevertheless it must be a matter of regret—and here the great master and author of modern mathematics has joined in expressing it—that so much less attention is now paid to

the Ancient Geometry than its beauty and clearness deserve; and if he could justly make this complaint a century and a half ago, when the old method had but recently, and only in part, fallen into neglect and disuse, how much more are such regrets natural in our day, when the very name of the Ancient Analysis has almost ceased to be known, and the beauties of the Greek Geometry are entirely veiled from the mathematician's eyes! It becomes, for this reason, necessary that the life of Simson, the great restorer of that geometry, should be prefaced by some remarks upon the nature of the science, in order that, in giving an account of his works, we may say his discoveries, it may not appear that we are recording the services of a great man to some science different from the mathematical.

The analysis of the Greek geometers was a method of investigation of peculiar elegance, and of no inconsiderable power. It consisted in supposing the thing as already done, the problem solved, or the truth of the theorem established; and from thence it reasoned until something was found, some point reached, by pursuing steps each one of which led to the next, and by only assuming things which were already known, having been ascertained by former discoveries. The thing thus found, the point reached, was the discovery of something which could by known methods be performed, or of something which, if not self-evident, was already by former discovery proved to be true; and in the one case a construction was thus found by which the problem was solved, in the other a proof was obtained that the theorem was true, because in both cases the ultimate point had been reached by strictly legitimate reasoning, from the assumption that the problem had been solved, or the assumption that the theorem was true. Thus, if it were required from a given point in a straight line given by position, to draw a straight line which should be cut by a given circle in segments, whose rectangle was equal to that of the segments of the diameter perpendicular to the given line—the thing is supposed to be done; and the equality of the rectangle gives a proportion between the

segments of the two lines, such that, joining the point supposed to be found, but not found, with the extremity of the diameter, the angle of that line with the line sought but not found, is shown by similar triangles to be a right angle, *i.e.*, the angle in a semicircle. Therefore the point through which the line must be drawn is the point at which the perpendicular cuts the given circle. Then, suppose the point given through which the line is to be drawn, if we find that the curve in which the other points are situate is a circle, we have a local theorem, affirming that, if lines be drawn through any point to a line perpendicular to the diameter, the rectangle made by the segments of all the lines cutting the perpendicular is constant; and this theorem would be demonstrated by supposing the thing true, and thus reasoning till we find that the angle in a semicircle is a right angle, a known truth. Lastly, suppose we change the hypothesis, and leave out the position of the point as given, and inquire after the point in the given straight line from which a line being drawn through a point to be found in the circle, the segments will contain a rectangle equal to the rectangle under the perpendicular segments—we find that one point answers this condition, but also that the problem becomes indeterminate: for every line drawn through that point to every point in the given straight line has segments, whose rectangle is equal to that under the segments of the perpendicular. The enunciation of this truth, of this possibility of finding such a point in the circle, is a *Porism*. The Greek geometers of the more modern school, or lower age, defined a Porism to be a proposition differing from a local theorem by a defect or defalcation in the hypothesis; and accordingly we find that this porism is derived from the local theorem formerly given, by leaving out part of the hypothesis. But we shall afterwards have occasion to observe that this is an illogical and imperfect definition, not coextensive with the thing defined; the above proposition, however, answers every definition of a Porism.

The demonstration of the theorem or of the construction obtained by investigation in this manner of proceeding, is

called *synthesis*, or *composition*, in opposition to the *analysis*, or the process of investigation: and it is frequently said that Plato imported the whole system in the visits which he made like Thales of Miletus and Pythagoras, to study under the Egyptian geometers, and afterwards to converse with Theodorus at Cyrene, and the Pythagorean School in Italy. But it can hardly be supposed that all the preceding geometers had worked their problems and theorems at random; that Thales and Pythagoras with their disciples, a century and a half before Plato, and Hippocrates, half a century before his time, had no knowledge of the analytical method, and pursued no systematic plan in their researches, devoted as their age was to geometrical studies. Plato may have improved and further systematised the method, as he was no doubt deeply impressed with the paramount importance of geometry, and even inscribed upon the gates of the Lyceum a prohibition against any one entering who was ignorant of it. The same spirit of exaggeration which ascribes to him the analytical method, has also given rise to the notion that he was the discoverer of the Conic Sections; a notion which is without any truth and without the least probability.

Of the works written by the Greek geometers some have come down to us; some of the most valuable, as the 'Elements' and 'Data' of Euclid, and the 'Conics' of Apollonius. Others are lost; but, happily, Pappus, a mathematician of some merit, who flourished in the Alexandrian school about the end of the fourth century, has left a valuable account of the geometrical writings of the elder Greeks. His work is of a miscellaneous nature, as its name, 'Mathematical Collections,' implies; and excepting a few passages, it has never been published in the original Greek. Commandini, of Urbino, made a translation of the whole six books then discovered; the first has never been found, but half the second being in the Savilian library at Oxford, was translated by Wallis a century later. Commandini's translation, with his learned commentary, was not printed before his death, but

the Duke of Urbino (Francesco Maria) caused it to be published in 1588, at Pisa, and a second edition was published at Venice the next year: a fact most honourable to that learned and accomplished age, when we recollect how many years Newton's immortal work was published before it reached a second edition, and that in the seventeenth and eighteenth centuries.

The two first books of Pappus appear to have been purely arithmetical, so that their loss is little to be lamented. The eighth is on mechanics, and the other five are geometrical. The most interesting portion is the seventh; the introduction of which, addressed to his son as a guide of his geometrical studies, contains a full enumeration of the works written by the Greek geometers, and an account of the particular subjects which each treated, in some instances giving a summary of the propositions themselves with more or less obscurity, but always with great brevity. Among them was a work which excited great interest, and for a long time baffled the conjectures of mathematicians, Euclid's three books of 'Porisms:' of these we shall afterwards have occasion to speak more fully. His 'Loci ad Superficiem,' apparently treating of curves of double curvature, is another, the loss of which was greatly lamented, the more because Pappus has given no account of its contents. This he had done in the case of the 'Loci Plani' of Apollonius. Euclid's four books on conic sections are also lost; but of Apollonius's eight books on the same subject, the most important of the whole series, the 'Elements' excepted, four were preserved, and three more were discovered in the seventeenth century. His Inclinations, his Tactions or Tangencies, his Sections of Space and of Ratio, and his Determinate Section, however curious, are of less importance; all of them are lost.

For many years Commandini's publication of the 'Collections' and his commentary did not lead to any attempt at restoring the lost works from the general account given by Pappus. Albert Girard, in 1634, informs us in a note to an edition of Stevinus, that he had restored Euclid's 'Porisms,' a

thing eminently unlikely, as he never published any part of his restoration, and it was not found after his decease. In 1637, Fermat restored the 'Loci Plani' of Apollonius, but in a manner so little according to the ancient analysis, that we cannot be said to approach by means of his labours the lost book on this subject. In 1615, De la Hire, a lover and a successful cultivator of the ancient method, published his Conic Sections, but synthetically treated; he added afterwards other works on epicycloids and conchoids, treated on the analytical plan. L'Hôpital, at the end of the seventeenth century, published an excellent treatise on Conics, but purely algebraical. At the beginning of the eighteenth century, Viviani and Grandi applied themselves to the ancient geometry; and the former gave a conjectural restoration (Divinatio) of Aristæus's 'Loci Solidi,' the curves of the second or Conic order. But all these attempts were exceedingly unsuccessful, and the world was left in the dark, for the most part, on the highly interesting subject of the Greek Geometry. We shall presently see that both Fermat and Halley, its most successful students, had made but an inconsiderable progress in the most difficult branches.

How entirely the academicians of France were either careless of those matters, or ignorant, or both, appears by the 'Encyclopédie;' the mathematical department of which was under no less a geometrician than D'Alembert. The definition there given of analysis, makes it synonymous with algebra: and yet mention is made of the ancient writers on analysis, and of the introduction to the seventh book of Pappus, with only this remark, that those authors differ much from the modern analysts. But the article 'Arithmetic' (vol. i. p. 677), demonstrates this ignorance completely; and that Pappus's celebrated introduction had been referred to by one who never read it. We there find it said, that Plato is supposed to have invented the ancient analysis; that Euclid, Apollonius, and others, including Pappus himself, studied it, but that we are quite ignorant of what it was: only that it is by some conceived to have resembled our algebra, or else Archimedes

could never have made his great geometrical discoveries. It is, certainly, quite incredible that such a name as D'Alembert's should be found affixed to this statement, which the mere reading of any one page of Pappus's books must have shown to be wholly erroneous; and our wonder is the greater, inasmuch as Simson's admirable restoration of Apollonius's 'Loci Plani' had been published five years before the 'Encyclopédie' appeared.

Again, in the 'Encyclopédie,' the word Analysis, as meaning the Greek method, and not algebra, is not even to be found. Nor do the words synthesis, or composition, inclinations, tactions or tangencies, occur at all; and though Porisms are mentioned, it is only to show the same ignorance of the subject; for that word is said to be synonymous with 'lemma,' because it is sometimes used by Pappus in the sense of subsidiary proposition.* When Clairault wrote his inestimable work on curves of double curvature, he made no reference whatever to Euclid's 'Loci ad Superficiem;' much less did he handle the subject after the same manner; he deals, indeed, with matters beyond the reach of the Greek Geometry.

Such was the state of this science when Robert Simson first applied to it his genius, equally vigorous and undaunted, with the taste which he had early imbibed for the beauty, the simplicity, and the closeness of the ancient analysis.

He was appointed professor in 1711, and taught with extraordinary success; but his genius was bent to the diligent investigation of truth, in the science of which he was so great a master. The ancient geometry, that of the Greeks of which I have spoken, early fixed his attention and occupied his mind by its extraordinary elegance, by the lucid clearness with which its investigations are conducted, by the exercise which it affords to the reasoning faculties, and above all, by the absolute rigour of its demonstrations. He never undervalued modern analysis; it is a great mistake to represent

* Euclid uses the word Corollary in his Elements.—See Note II.

him as either disliking its process, or insensible to its vast importance for the solution of questions which the Greek analysis is wholly incapable of reaching. But he considered it as only to be used in its proper sphere; and that sphere he held to exclude whatever of geometrical investigation can be, with convenience and elegance, carried on by purely geometrical methods. The application of algebra to geometry, it would be ridiculous to suppose that either he or his celebrated pupil Matthew Stewart disliked or undervalued. That application forms the most valuable service which modern analysis has rendered to science. But they did object, and most reasonably and consistently, to the introduction of algebraic reasoning wherever the investigation could, though less easily, yet far more satisfactorily, be performed geometrically. They saw, too, that in many instances the algebraic solution leads to constructions of the most complex, clumsy, unmanageable kind, and therefore must be, in all these instances, reckoned more difficult, and even more prolix than the geometrical, from the former being confined to the expression of all the relations of space and position, by magnitudes, by quantity and number (even after the arithmetic of sines had been introduced), while the latter could avail itself of circles and angles directly. They would have equally objected to carrying geometrical reasoning into the fields peculiarly appropriate to modern analysis; and if one of them, Stewart, did endeavour to investigate by the ancient geometry physical problems supposed to be placed beyond its reach—as the sun's distance, in which he failed, and Kepler's problem, in which he marvellously succeeded, that of dividing the elliptical area in a given ratio by a straight line drawn from one focus—this is to be taken only as an homage to the undervalued potency of the Greek analysis, or at most, as a feat of geometrical force, and by no means as an indication of any wish to substitute so imperfect, however beautiful, an instrument, for the more powerful, though more ordinary one of the calculus which "alone can work great marvels." At the same time, and with all the necessary confession of the

merits of the modern method, it is certain that those geometricians would have regarded the course taken by some of its votaries in more recent times as exceptionable, whether with a view to clearness or to good taste: a course to the full as objectionable as would be the banishing of algebraical and substituting of geometrical symbols in the investigations of the higher geometry. La Place's great work, the 'Mécanique Céleste,' and La Grange's 'Mécanique Analytique,' have treated of the whole science of dynamics and of physical astronomy, comprehending all the doctrine of trajectories, dealing with geometrical ideas throughout, and ideas so purely geometrical that the algebraic symbols, as far as their works are concerned, have no possible meaning apart from lines, angles, surfaces; and yet in their whole compass they have not one single diagram of any kind. Surely, we may ask if $\frac{y}{dy}\sqrt{dx^2+dy^2}$, $\frac{ds^3}{dx^2\,d\left(\frac{dy}{dx}\right)}$* can possibly bear any other meaning than the tangent and the radius of curvature of a curve line: that is, a straight line touching a curve, and a circle whose curvature is that of another curve where they meet; any meaning, at least, which can make it material that they should ever be seen on the page of the analyst. These expressions are utterly without sense, except in reference to geometrical considerations; for although x and y are so general that they express any numbers, any lines, nay, any ideas, any rewards or punishments, any thoughts of the mind, it is manifest that the square of the differential of a thought, or the differential of the differential of a reward or punishment, has no meaning; and so of everything else but of the very tangent or the osculating circle's radius: consequently the generality of the symbols is wholly useless; the particular case of two lines being the only thing to which

* Or $\frac{(dx^2+dy^2)^{\frac{3}{2}}}{dx^2\,d\left(\frac{dy}{dx}\right)}$.

the expressions can possibly be meant to apply. Why, then, all geometrical symbols should be so carefully avoided when we are really treating of geometrical examples and geometrical ideas, and of these alone, seems hard to understand.

As the exclusive lovers of modern analysis have frequently and very erroneously suspected the ancients of possessing some such instrument, and concealing the use of it by giving their demonstrations synthetically after reaching their conclusions analytically, so some lovers of ancient analysis have supposed that Sir Isaac Newton obtained his solutions by algebraic investigations, and then covered them with a synthetic dress. Among others, Dr. Simson leant to this opinion respecting the 'Principia.' He used to say that he knew this from Halley, by whose urgent advice Sir Isaac was induced to adopt the synthetic form of demonstration, after having discovered the truths analytically. Machin is known to have held the same language; he said that the 'Principia' was algebra in disguise. Assuredly, the probability of this is far greater than that of the ancients having possessed and kept secret the analytical process of modern times. In the preface to his 'Loci Plani,' Dr. Simson fully refutes this notion respecting the ancients: a notion which, among others, no less a writer than Wallis had strongly maintained.*

That he did not undervalue algebra and the calculus to

* Algebra Præf. "Hanc Græcos olim habuisse non est quod dubitemus; sed studio celatam, nec temere propalandam. Ejus effectus (utut clam celatæ) satis conspicui apud Archimedem, Apollonium, aliosque." It is strange that any one of ordinary reflection should have overlooked the utter impossibility of all the geometricians in ancient times keeping the secret of an art which must, if it existed, have been universally known in the mathematical schools, and at a time when every man of the least learning, or even of the most ordinary education, was taught geometry. Montucla touches on this subject, but not with his wonted accuracy, (I. 166). Indeed, he seems here to confound ancient with modern analysis, although no one has more accurately described and illustrated the ancient method, (I. 164, 275). He adopts the erroneous notion of Plato having discovered this method; but he does not fall into the other error of ascribing to him the discovery of Conic Sections, (*ib.* 168).

which it has given rise, appears from many circumstances—among others, from what has already been stated; it appears also from this, that in many of his manuscripts there are found algebraical formulas for propositions which he had investigated geometrically. Maclaurin consulted him on the preparation of his admirable work, the 'Fluxions,' and received from him copious suggestions and assistance. Indeed, he adopted from him the celebrated demonstration of the fluxion (or differential) of a rectangle.* But Simson's whole mind, when left to its natural bent, was given to the beauties of the Greek Geometry; and he had not been many months settled in his academical situation when he began to follow the advice which Halley had given him, as both calculated, he said, to promote his own reputation, and to confer a lasting benefit upon the science cultivated by them both with an equal devotion. It is even certain that the obscure and most difficult subject of Porisms very early occupied his thoughts, and was the field of his researches, though to the end of his life he never had made such progress in the investigation as satisfied himself. Before 1715, three years after he began his course of teaching, he was deeply engaged in this inquiry; but he only regarded it as one branch of the great and dark subject which Halley had recommended to his care. After he had completely examined, corrected, and published, with most important additions, the Conics of Apollonius, which happily remain entire, but which, as we have seen, had been most inelegantly and indeed algebraically given by De la Hire, L'Hôpital, and others, to restore the lost books was his great desire, and formed the grand achievement which he set before his eyes.

We have already shown how scanty the light was by which his steps in this path must be guided. The introduction to the Seventh book of Pappus contained the whole that had reached our times to let us know the contents of the lost works. Some of the summaries which that valuable discourse

* Book i. chap. ii. prop. 3.

contains are sufficiently explicit, as those of the Loci Plani and the Determinate Section. Accordingly, former geometricians had succeeded in restoring the Loci Plani, or those propositions which treat of loci to the circle and rectilinear figures. They had, indeed, proceeded in a very unsatisfactory manner. Schooten, a Dutch mathematician of great industry and no taste, had given purely algebraic solutions and demonstrations. Fermat, one of the greatest mathematicians of the seventeenth century, had proceeded more according to the geometrical rules of the ancients; but he had kept to general solutions, and neither he nor Schooten had given the different cases, according as the data in each proposition were varied; so that their works were nearly useless in the solution of problems, the great purpose of Apollonius, as of all the authors of the τοπος αναλυομενου — the thirty-three ancient books. As for the analysis, it was given by neither, unless, indeed, Schooten's algebra is to be so termed. Fermat's demonstrations were all synthetical. His treatise, though written as early as 1629, was only published among his collected works in 1670. Schooten's was published among his 'Exercitationes Mathematicæ' in 1657. Of the field thus left open, Dr. Simson took possession, and he most successfully cultivated every corner of it. Nothing is left without the most full discussion; all the cases of each proposition are thoroughly investigated. Many new truths of great importance are added to those which had been unfolded by the Greek philosopher. The whole is given with the perfect precision and the pure elegance of the ancient analysis; and the universal assent of the scientific world has even confessed that there is every reason to consider the restored work as greatly superior to the lost original.

The history of this excellent treatise shows in a striking manner the cautious and modest nature of its author. He had completed it in 1738; but, unsatisfied with it, he kept it by him for eight years. He could not bring himself to think that he had given the "ipsissimæ propositiones of Apollonius in the very order and spirit of the original work." He was

tnen persuaded to let the book appear, and it was published in 1746. His former scruples and alarms recurred; he stopped the publication; he bought up the copies that had been sold; he kept them three years longer by him; and it was only in 1749 that the work really appeared. Thus had a geometrician complied with the rule prescribed by Horace for those who have no standard by which to estimate with exactness the merit of their writings.

In the meantime he had extended his researches into other parts of the subject. Among the rest he had restored and greatly extended the work on Determinate Section, or the various propositions respecting the properties of the squares and rectangles of segments of lines passing through given points. There is no doubt that the prolixity, however elegant, with which the ancients treated this subject, is somewhat out of proportion to its importance; and as it is peculiarly adapted to the algebraical method, presenting, indeed, little difficulty, to the analyst, the loss of the Pergæan treatise is the less to be deplored, and its restoration was the less to be desired. Apollonius had even thought it expedient to give a double set of solutions; one by straight lines, the other by semicircles. Dr. Simson's restoration is most full, certainly, and contains many and large additions of his own. It fills above three hundred quarto pages. His predecessors had been Snellius, whose attempt, published in 1608, was universally allowed to be a failure; and Anderson, a professor of Aberdeen, whose work, in 1612, was much better, but confined to a small part only of the subject.

About the time that Dr. Simson finally published the Loci Plani, he began his great labour of giving a correct and full edition of the Elements. The manner in which this has been accomplished by him is well known. The utmost care was bestowed on the revision of the text; no pains were spared in collating editions; all commentaries were consulted; and the elegance and perfect method of the original has been so admirably preserved, that no rival has ever yet risen up to dispute with Simson's Euclid the possession of the schools.

The time bestowed on this useful work was no less than nine years. It only was published in 1758. To the second edition, in 1762, he added a similarly correct edition of the Data, comprising several very valuable original propositions of his own, of Mr. Stewart, and of Lord Stanhope, together with two excellent problems to illustrate the use of the Data in solutions.

We thus find Dr. Simson employed in these various works which he successively gave to the world, elaborated with infinite care, and of which the fame and the use will remain as long as the mathematics are cultivated; some of them delighting students who pursue the science for the mere speculative love of contemplating abstract truths, and the gratification of following the rigorous proofs peculiar to that science; some for the instruction of men in the elements, which are to form the foundation of their practical applications of geometry. But all the while his mind never could be wholly weaned from the speculation which had in his earliest days riveted his attention by its curious and singular nature, and fired his youthful ambition by its difficulty, and its having vanquished all his predecessors in their efforts to master it. We have seen that as early as 1715 at the latest, probably much earlier, the obscure subject of Porisms had engaged his thoughts; and soon after, his mind was so entirely absorbed by it that he could apply to no other investigation. The extreme imperfection of the text of Pappus; the dubious nature of his description; his rejection of the definition which appeared intelligible; his substituting nothing in its place except an account so general that it really conveyed no precise information; the hiatus in the account which he subjoins of Euclid's three books, so that even with the help of the lemmas related to these propositions of the lost work, no clear or steady light could be descried to guide the inquirer—for the first porism of the first book alone remained entire, the general porism being given wholly truncated (mancum et imperfectum)—all seemed to present obstacles wholly insurmountable; and after various attempts for

years he was fain to conclude with Halley that the mystery belonged to the number of those which can never be penetrated. He lost his rest in the anxiety of this inquiry; sleep forsook his couch; his appetite was gone; his health was wholly shaken; he was compelled to give over the pursuit; he was "obliged," he says, "to resolve steadily that he never more should touch the subject, and as often as it came upon him he drove it away from his thoughts." *

It happened, however, about the month of April, 1722, that while walking on the banks of the Clyde with some friends, he had fallen behind the company; and musing alone, the rejected topic found access to his thoughts. After some time a sudden light broke in upon him; it seemed at length as if he could descry something of a path, slippery, tangled, interrupted, but still practicable, and leading at least in the direction towards the object of his research. He eagerly drew a figure on the stump of a neighbouring tree with a piece of chalk; he felt assured that he had now the means of solving the great problem; and although he afterwards tells us that he then had not a sufficiently clear notion of the subject (eo tempore Porismatum naturam non satis compertam habebam),† yet he accomplished enough to make him communicate a paper upon the discovery to the Royal Society, the first work he ever published (Phil. Trans. for 1723). He was wont in after life to show the spot on which the tree, long since decayed, had stood. If peradventure it had been preserved, the frequent lover of Greek Geometry would have been seen making his pilgrimage to a spot consecrated by such touching recollections. The graphic pen of Montucla, which gave such interest to the story of the first observation of the transit of Venus by Horrox in Lancashire, and to the Torricellian experiment, ‡ is alone wanting to clothe this passage in colours as vivid and as unfading.

* "Firmiter animum induxi hæc nunquam in posterum investigare. Unde quoties menti occurrebant, toties eas arcebam."—(Op. Rel. 320. Præf. ad Porismata.)

† Op. Rel. 320.

‡ Hist. de Math. vol. i.

This great geometrician continued at all the intervals of his other labours intently to investigate the subject on which he thus first threw a steady light.

His first care upon having made this discovery was to extend the particular propositions until he had obtained the general one. A note among his memoranda appears to have been made, according to his custom, of marking the date at which he succeeded in any of his investigations.*—"Hodie hæc de porismatis inveni, R. S., 23 April, 1722." Another note, 27th April, 1722, shows that he had then obtained the general proposition; he afterwards communicated this to Maclaurin when he passed through Glasgow on his way to France; and he, on his return, communicated to Dr. Simson without demonstration a proposition concerning conics derived from what he had shown him—a proposition which led his friend to insert some important investigations in his Conic Sections. In 1723 the publication of his paper took place in the 'Philosophical Transactions;' it is extremely short, and does not appear to contain all that the author had communicated; for we find this sentence inserted before the last portion of the paper:—"His adjecit clarrissimus professor propositiones duas sequentes libri primi Porismatum Euclidis, a se quoque restitutas." The paper contains the first general proposition and its ten cases, and then the second with its cases. No general description or definition is given of Porisms; and it is plain that his mind was not then finally made up on this obscure subject, although he had obtained a clear view of it generally.

At what time his knowledge of the whole became matured we are not informed; but we know that his own nature was

* In one there is this note upon the solution of a problem of tactions,—"Feb 9, 1734:—Post horam primam ante meridiem;" and much later in life we find the same particularity in marking the time of discovery. His birthday was October 14, and having solved a problem on that day 1764, he says—

14 Octobr. 1764.
14 Octobr. 1687.

Deo Opt. Max. benignissimo Servatori
Laus et gloria.

77 (scil. Anno Ætatis.)

nice and difficult on the subject of his own works; that he never was satisfied with what he had accomplished; and he probably went on making constant additions and improvements to his work. Often urged to publish, he as constantly refused; indeed, he would say that he had done nothing, or next to nothing, which was in a state to appear before the world; and moreover, he very early began to apprehend a decay of his faculties, from observing his recollection of recent things to fail, as is very usual with all men; for as early as 1751, we find him giving this as a reason for declining to undertake a work on Lord Stanhope's recommendation, when he was only in his sixty-fifth year. Thus, though he at first used to say he had nothing ready for publication, he afterwards added, that he was too old to complete his work satisfactorily. In his earlier days he used occasionally to affect a kind of odd mystery on the subject, and when one of his pupils (Dr. Traill) submitted to him some propositions, which he regarded as porisms, Dr. Simson would neither admit nor deny that they were such, but said with some pleasantry, "They are propositions." One of them, however, he has given in his work as a porism, and with a complimentary reference to its ingenious and learned author.

Thus his life wore away without completing this great work, at least without putting it in such a condition as satisfied himself. It was left among his MSS., and by the judicious munificence of a noble geometrician, the liberal friend of scientific men, as well as a successful cultivator of science, Earl Stanhope,* it was, after his death, published, with his restoration of Apollonius' treatise De Sectione determinatâ, a short paper on Logarithms, and another on the Method of Limits geometrically demonstrated, the whole forming a very handsome quarto volume; of which the Porisms occupy nearly one-half, or 277 pages.

This work is certainly the master-piece of its distinguished

* Grandfather of the present Earl, whose father also was a successful cultivator of natural science, mechanical especially.

author. The extreme difficulty of the subject was increased by the corruptions of the text that remains in the only passage of the Greek geometers which has reached us, the only few sentences in which any mention whatever is made of Porisms. This passage is contained in the preface or introduction to the Seventh book of Pappus, which we have already had occasion to cite. But this was by far the least of the difficulties which met the inquirer after the hidden treasure, the restorer of lost science, though Albert Girard thought or said, in 1635, that he had restored the Porisms of Euclid. As we have seen, no trace of his labours is left; and it seems extremely unlikely that he should have really performed such a feat and given no proofs of it. Halley, the most learned and able of Dr. Simson's predecessors, had tried the subject, and tried it in vain. He thus records his failure:—"Hactenus Porismatum descriptio nec mihi intellecta nec lectori profutura." These are his words, in a preface to a translation which he published of Pappus's Seventh book, much superior in execution to that of Commandini. But this eminent geometrician was much more honest than some, and much more safe and free from mistake than others who touched upon the subject that occupied all students of the ancient analysis. He was far from pretending, like Girardus, to have discovered that of which all were in quest. But neither did he blunder like Pemberton, whom we find, the very year of Simson's first publication, actually saying in his paper on the Rainbow—"For the greater brevity I shall deliver them (his propositions) in the form of porisms, as, in my opinion, the ancients called all propositions treated by analysis only" (Philosophical Transactions, 1723, p. 148); and, truth to say, his investigation is not very like ancient analysis either. The notion of D'Alembert, somewhat later, has been alluded to already; he imagined porism to be synonymous with lemma, misled by an equivocal use of the word in some passages of ancient authors, if indeed he had ever studied any of the writers on the Greek Geometry, which, from what I have stated before, seems exceedingly doubtful. But the

most extraordinary, and indeed inexcusable ignorance of the subject is to be seen in some who, long after Simson's paper had been published, were still in the dark; and though that paper did not fully explain the matter, it yet ought to have prevented such errors as these fell into. Thus Castillon, in 1761, showed that he conceived porisms to be merely the constructions of Euclid's Data. If this were so, there might have been some truth in his boast of having solved all the Porisms of Euclid; and he might have been able to perform his promise of soon publishing a restoration of those lost books.

It is remarkable enough that before Halley's attempts and their failure, candidly acknowledged by himself, Fermat had made a far nearer approach to a solution of the difficulty than any other of Simson's predecessors. That great geometrician, after fully admitting the difficulty of the subject, and asserting* that, in modern times, porisms were known hardly even by name, announces somewhat too confidently, if not somewhat vaingloriously, that the light had at length dawned upon him,† and that he should soon give a full restoration of the whole three lost books of Euclid. Now the light had but broke in by a small chink, as a mere faint glimmering, and this restoration was quite impossible, inasmuch as there remained no account of what those books contained, excepting a very small portion obscurely mentioned in the preface of Pappus, and the lemmas given in the course of the Seventh book, and given as subservient to the resolution of porismatic questions. Nevertheless, Fermat gave a demonstration of five propositions, "in order," he says, "to show what a porism is, and to what purposes it is subservient." These propositions are, indeed, porisms, though their several

* "Intentata ac velut disperata Porismatum Euclidæa doctrina.—Geometrici (ævi recentioris) nec vel de nomine cognoverunt, aut quod esset solummodo sunt suspicati."—(Var. Opera, p. 166.)

† "Nobis in tenebris dudum cæcutientibus tandem se (Natura Porismatum) clara ad videndum obtulit, et purâ per noctem luce refulsit."—(Epist. ib.)

enunciations are not given in the true porismatic form. Thus, in the most remarkable of them, the fifth, he gives the construction as part of the enunciation. So far, however, a considerable step was made; but when he comes to show in what manner he discovered the nature of his porisms, and how he defines them, it is plain that he is entirely misled by the erroneous definition justly censured in the passage of Pappus already referred to. He tells us that his propositions answer the definition; he adds that it reveals the whole nature of porisms; he says that by no other account but the one contained in the definition, could we ever have arrived at a knowledge of the hidden value;* and he shows how, in his fifth proposition, the porism flows from a locus, or rather he confounds porisms with loci, saying porisms generally are loci, and so he treats his own fifth proposition as a locus; and yet the locus to a circle which he states as that from which his proposition flows has no connexion with it, according to Dr. Simson's just remark ('Opera Reliqua,' p. 345). That the definition on which he relies is truly imperfect, appears from this: there could be no algebraical porism, were every porism connected with a local theorem. But an abundant variety of geometrical porisms can be referred to, which have no possible connexion with loci. Thus, it has never been denied that most of the Propositions in the Higher Geometry, which I investigated in 1797, were porisms, yet many of them were wholly unconnected with loci; as that affirming the possibility of describing an hyperbola which should cut in a given ratio all the areas of the parabolas lying between given straight lines.† Here the locus has nothing to do with the solution, as if the proposition were a kind of a local theorem: it is only the line dividing the curvilineal areas, and it divides innumerable such areas. Professor Playfair, who had thoroughly investigated the whole subject, never in considering this proposition doubted for a moment its being most strictly a porism.

* Var. Op. p. 118.

† Phil. Trans. 1798, p. 111. Tract I. of this volume.

Therefore, although Fermat must be allowed to have made a considerable step, he was unacquainted with the true nature of the porism; and instead of making good his boast that he could restore the lost books, he never even attempted to restore the investigation of the first proposition, the only one that remains entire. A better proof can hardly be given of the difficulty of the whole subject.*

Indeed it must be confessed that Pappus's account of it, our only source of knowledge, is exceedingly obscure, all but the panegyric which in a somewhat tantalizing manner, he pronounces upon it. "Collectio," says he, "curiosissima multarum rerum spectantium ad resolutionem difficiliorum et generaliorum problematum" (lib. vii. Proem). His definition already cited is, as he himself admits, very inaccurate; because the connexion with a locus is not necessary to the porismatic nature, although it will very often exist, inasmuch as each point in the curve having the same relation to certain lines, its description will, in most cases, furnish the solution of a problem, whence a porism may be deduced. Nor does Pappus, while admitting the inaccuracy of the definition, give us one of his own. Perhaps we may accurately enough define a porism to be the enunciation of the possibility of finding that case in which a determinate problem becomes indeterminate, and admits of an infinity of solutions, all of which are given by the statement of the case.

For it appears essential to the nature of a porism that it should have some connexion with an indeterminate problem and its solution. I apprehend that the poristic case is always one in which the data become such that a transition is made from the determinate to the indeterminate, from the problem

* The respect due to the great name of Fermat, a venerable magistrate and most able geometrician, is not to be questioned. He was, indeed, one of the first mathematicians of the age in which he flourished, along with the Robervals, the Harriots, the Descartes. How near he approached the differential calculus is well known. His correspondence with Roberval, Gassendi, Pascal, and others, occupies ninety folio pages of his posthumous works, and contains many most ingenious, original, and profound observations on various branches of science.

being capable of one or two solutions, to its being capable of an infinite number. Thus it would be no porism to affirm that an ellipse being given, two lines may be found at right angles to each other, cutting the curve, and being in a proportion to each other which may be found: the two lines are the perpendiculars at the centre, and are of course the two axes of the ellipse; and though this enunciation is in the outward form of a porism, the proposition is no more a porism than any ordinary problem; as that a circle being given, a a point may be found from whence all the lines drawn to the circumference are equal, which is merely the finding of the centre. But suppose there be given the problem to inflect two lines from two given points to the circumference of an ellipse, the sum of which lines shall be equal to a given line, the solution will give four lines, two on each side of the transverse axis. But in one case there will be innumerable lines which answer the conditions, namely, when the two points are in the axis, and so situated that the distance of each of them from the farthest extremity of the axis is equal to the given line, the points being the foci of the ellipse. It is, then, a porism to affirm that an ellipse being given, two points may be found such that if from them be inflected lines to any point whatever of the curve, their sum shall be equal to a straight line which may be found; and so of the Cassinian curve, in which the rectangle under the inflected lines is given. In like manner if it be sought in an ellipse to inflect from two given points in a given straight line, two lines to a point in the curve, so that the tangent to that point shall, with the two points and the ordinate, cut the given line in harmonical ratio; this, which is only capable of one solution in ordinary cases, becomes capable of an infinite number when the two points are in the axis, and when the ellipse cuts it; for in that case every tangent that can be drawn, and every ordinate, cut the given line harmonically with the curve itself.*

* The ellipse has this curious property, which I do not find noticed by Maclaurin in his Latin Treatise on Curve Lines appended to the Algebra,

Dr. Simson's definition is such that it connects itself with an indeterminate case of some problem solved; but it is defective, in appearance rather than in reality, from seeming to confine itself to one class of porisms. This appearance arises from using the word "*given*" (*data* or *datum*) in two different senses, both as describing the hypothesis and as affirming the possibility of finding the construction so as to answer the conditions. This double use of the word, indeed, runs through the book, and though purely classical, is yet very inconvenient; for it would be much more distinct to make one class of things those which are assuredly data, and the other, things which may be found. Nevertheless, as his definition makes all the innumerable things not given have the same relation to those which are given, this should seem to be a limitation of the definition not necessary to the poristic nature. Pappus's definition, or rather that which he says the ancients gave, and which is not exposed to the objection taken by him to the modern one, is really no definition at all; it is only that a porism is something between a theorem and a problem, and in which, instead of anything being proposed to be done, or to be proved, something is proposed to be investigated. This is erroneous, and contrary to the rules of

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and dealing a good deal with Harmonical proportions. If from any point whatever out of the ellipse there be drawn a straight line in any direction whatever cutting the ellipse, the line is cut harmonically by the tangent, the ordinate, and the chords of the two arcs intercepted between the point of contact of the tangent and the axis. The tangent, sine, and chords are always an harmonical pencil, and consequently cut in the Harmonical ratio all lines drawn in all directions, from the given point. This applies to all ellipses upon the same axis, (all having the same subtangent,) and of course to the circle. The ellipse, therefore, might be called the *Harmonical Curve*, did not another of the 12th order rather merit that name, which has its axis divided harmonically by the tangent, the normal, the ordinate, and a given point in the axis. Its differential equation is $2\,dy^2 + dx^2 = \frac{y\,dy\,dx}{x}$, which is reducible, and its integral is an equation of the 12th order. There is also another Harmonical Curve, a transcendental one, in which chords vibrate isochronously.

logic from its generality; it is, as the lawyers say, void for uncertainty. The modern one objected to by Pappus is not uncertain; it is quite accurate as far as it goes; but it is too confined, and errs against the rules of logic by not being coextensive with the thing proposed to be defined.

The difficulty of the subject has been sufficiently shown from the extreme conciseness and the many omissions, the almost studied obscurity, of the only account of it which remains; and to this must certainly be added the corruption of the Greek text. The success which attended Dr. Simson's labours in restoring the lost work, as far as that was possible, and, at any rate, in giving a full elucidation of the nature of porisms, now, for the first time, disclosed to mathematicians, is, on account of those great difficulties by which his predecessors had been baffled, the more to be admired. But there is one thing yet more justly a matter of wonder, when we contrast his proceedings with theirs. The greater part of his life, a life exclusively devoted to mathematical study, had been passed in these researches. He had very early become possessed of the whole mystery, from other eyes so long concealed. He had obtained a number of the most curious solutions of problems connected with porisms, and was constantly adding to his store of porisms and of lemmas subservient to their investigation. For many years before his death, his work had attained, certainly the form, if not the size, in which we now possess it. Yet he never could so far satisfy himself with what has abundantly satisfied every one else, as to make it public, and he left it unpublished among his papers when he died. Nothing can be more unlike those who freely boasted of having discovered the secret, and promised to restore the whole of Euclid's lost books. It is as certain that the secret was never revealed to them as it is that neither they nor any man could restore the books. But how speedily would the Castillons, the Girards, even the Fermats, have given their works to the world had they become possessed of such a treasure as Dr. Simson had found! Yet though ready for the press, and with its preface composed, and its title

given in minute particularity, he never could think that he had so far elaborated and finished it as to warrant him in finally resolving on its publication.

There needs no panegyric of this most admirable performance. Its great merit is best estimated by the view which has been taken of the extraordinary difficulties overcome by it. The difficulty of some investigations—the singular beauty of the propositions, a beauty peculiar to the porism from the wonderfully general relations which it discloses—the simplicity of the combinations—the perfect elegance of the demonstrations—render this a treatise in which the lovers of geometrical science must ever find the purest delight.

Beside the general discussions in the preface, and in a long and valuable scholium after the sixth proposition, and an example of algebraical porisms, Dr. Simson has given in all ninety-one propositions. Of these, four are problems, ten are loci, forty-three are theorems, and the remaining thirty-four are porisms, including four suggested by Matthew Stewart, and the five of Fermat improved and generalized; there are, besides, four lemmas and one porism suggested by Dr. Traill, when studying under the professor. There may thus be said to be in all ninety-eight propositions. The four lemmas are propositions ancillary to the author's own investigations; for many of his theorems are the lemmas preserved by Pappus as ancillary to the porisms of Euclid.

In all these investigations the strictness of the Greek geometry is preserved almost to an excess; and there cannot well be given a more remarkable illustration of its extreme rigour than the very outset of this great work presents. The porism is, that a point may be found in any given circle through which all the lines drawn cutting its circumference and meeting a given straight line shall have their segments within and without the circle in the same ratio. This, though a beautiful proposition, is one very easily demonstrated, and is, indeed, a corollary to some of those in the 'Elements.' But Dr. Simson prefixes a lemma: that the line drawn to the right angle of a triangle from the middle point of the

hypotenuse, is equal to half that hypotenuse. Now this follows, if the segment containing the right angle be a semicircle, and it might be thought that this should be assumed only as a manifest corollary from the proposition, or as the plain converse of the proposition, that the angle in a semicircle is a right angle, but rather as identical with that proposition; for if we say the semicircle is a right-angled segment, we also say that the right-angled segment is a semicircle. But then it might be supposed that two semicircles could stand on one base; or, which is the same thing, that two perpendiculars could be drawn from one point to the same line; and as these propositions had not been in the elements (though the one follows from the definition of the circle, and the other from the theorem that the three angles of a triangle are equal to two right angles), and as it might be supposed that two or more circles, like two or more ellipses, might be drawn on the same axis, therefore the lemma is demonstrated by a construction into which the centre does not enter. Again, in applying this lemma to the porism (the proportion of the segments given by similar triangles), a right angle is drawn at the point of the circumference, to which a line is drawn from the extremity of a perpendicular to the given line; and this, though it proves that perpendicular to pass through the centre, unless two semicircles could stand on the same diameter, is not held sufficient; but the analysis is continued by help of the lemma to show that the perpendicular to the given line passes through the centre of the given circle, and that therefore the point is found. It is probable that the author began his work with a simple case, and gave it a peculiarly rigorous investigation in order to explain, as he immediately after does clearly in the scholium already referred to, the nature of the porism, and to illustrate the erroneous definitions of later times (*νεωτερικοι*) of which Pappus complains as illogical.

Of porisms, examples have been now given both in plain geometry, in solid, and in the higher: that is, in their connexion both with straight lines and circles, with conic sec-

tions, and with curves of the third and higher orders. Of an algebraical porism it is easy to give examples from problems becoming indeterminate; but these propositions may likewise arise from a change in the conditions of determinate problems. Thus, if we seek for a number, such that its multiple by the sum of two quantities shall be equal to its multiple by the difference of these quantities, together with twice its multiple by a third given quantity, we have the equation $(a+b)\,x=(a-b)\,x+2\,c\,x$ and $2b\,x=2\,c\,x$; in which it is evident that if $c=b$, any number whatever will answer the conditions, and thus we have this porism: Two numbers being given a third may be found, such that the multiple of any number whatever by the sum of the given numbers, shall be equal to its multiple by their differences, together with half its multiple by the number to be found. That number is in the ratio of 4 : 3 to the lesser given number.

There are many porisms also in dynamics. One relates to the centre of gravity which is the porismatic case of a problem. The porism may be thus enunciated:—Any number of points being given, a point may be found such, that if any straight line whatever be drawn through it, the sum of the perpendiculars to it from the points on one side will be equal to the sum of the perpendiculars from the points on the other side. That point is consequently the centre of gravity: for the system is in equilibrium by the proposition. Another is famous in the history of the mixed mathematics. Sir Isaac Newton, by a train of most profound and ingenious investigation, reduced the problem of finding a comet's place from three observations (a problem of such difficulty, that he says of it, "hocce problema longe difficilimum omnimodo aggressus,"*) to the drawing a straight line through four lines given by position, and which shall be cut by them in three segments having given ratios to each other. Now his solution of this problem, the corollary to the twenty-seventh lemma of the first book, has a porismatic case, that is, a case in which

* Principia, lib. iii. prop. xli.

any line that can be drawn through the given lines will be cut by them in the same proportions, like the lines drawn through three harmonicals in the porism already given of the harmonical curve. To this Newton had not adverted, nor to the unfortunate circumstance that the case of comets is actually the case in which the problem thus becomes capable of an infinite number of solutions. The error was only discovered after 1739, when it was found that the comet of that year was thrown on the wrong side of the sun by the Newtonian method. This enormous discrepancy of the theory with observation, led to a full consideration of the subject, and to a discovery of the porismatic case.*

* The remarkable circumstance of the case of the comet's motion, for which Sir I. Newton's solution was intended, proving to be the porismatic case of the construction, has been mentioned in the text. It has been sometimes considered as singular, that this did not occur to himself, the more especially as he evidently had observed two cases in which the problem became indeterminate—namely, when the lines were parallel, and when they all met in one point, for he excepts those cases in express terms (*Prin. lib.* 1. *Lem.* xxvii.). It may be observed, that such oversights could very rarely happen to the ancient geometers, because they most carefully examined each variation in the data, and so gave to their solutions such a fulness as exhausted the subject.

The commentators on the Principia (Le Seur and Jacquier) make no mention of the omission. The circumstance of the porismatic case was not discovered till ten years after their publication, when F. Boscovich found it out, in 1749. But it is very extraordinary that Montucla appears to have been unaware of the matter, although the first edition of his work did not appear till 1758. Nor is the least reference made to it in the second edition, which was published the year he died (1799). There are other omissions in both editions, and also in the continuation. He appears well to have understood the ancient method, and to have read and examined some of the most celebrated works upon it. He had given due praise to Simson in his first edition; and to Lord Stanhope, who sent him the 'Opera Reliqua;' and we find in the second edition a full note upon the subject, ii. 277. In the continuation—iii. 11, and seq., we have further indications of the attention which he had bestowed upon the ancient geometry; but it is remarkable that though Matthew Stewart's Tracts, published in 1761, were known to him, he was wholly unacquainted with the 'Propositiones Geometricæ,' which appeared soon after, and with the General Theorems which had been published fifteen years

before. Nor does he appear to have even seen Professor Playfair's admirable paper upon Porisms in the Edinburgh Transactions, 1794, the war having probably impeded the intercourse of the two countries. Had he seen this, he must have been brought acquainted with the history of the porism relating to the comet's place, for it is there fully given.

It must be added, that Montucla's mathematical pursuits had for many years been interrupted by the duties of the places which he held under the government, until the Revolution (Pref. 111); and although the loss of those employments restored him to his studies, it is probable that he rather applied himself to the continuation of the History, the bringing it down from the period to which the first volume extended, than to supply omissions in those volumes, considerable as are the additions which he made to them.

The third and fourth volumes were not published till after his death, which happened when only a third part of the former had been printed. Lalande undertook the revision of the rest, and how great soever his merits may have been as a practical astronomer, as an author, and a teacher of astronomy, he had none of the mathematical acquirements which could fit him for superintending the publication of Montucla's work. He had some assistance from a very eminent mathematician, Lacroix, and the notes given by him are, as might be expected, excellent. But we are not distinctly informed of the additions, if any, which he made to the text, while there appears considerable reason to suppose that Lalande sometimes interfered with it. Certain it is, that many things would have been suppressed, and others added, had Montucla survived to finish the work of correcting and publishing. There is no reason to think that the eminent analyst referred to (Lacroix), would have supplied Montucla's omissions regarding the poristic case in the Principia, or regarding the writers on the ancient analysis; for on this subject he was much better informed, in all probability, than Lacroix, and the omission in the Principia comes less within the scope of modern than ancient geometry.*

* This tract is taken from 'Lives of Philosophers,'—Life of Simson.

V.

SUR CERTAINS PARADOXES RÉELS OU SUPPOSÉS, PRINCIPALEMENT DANS LE CALCUL INTÉGRAL.

L'EXAMEN des paradoxes, dont l'existence a été fréquemment supposée, est d'une grande importance, parce que si la supposition a été sans fondement, la doctrine est délivrée de la charge d'inconséquence; et si les difficultés ne reçoivent point de solution satisfaisante, nous pouvons nous assurer que l'on est arrivé à quelque vérité nouvelle, ou à quelque limitation importante des propositions généralement admises. On trouvera pourtant que ce chapitre (qui pourra être appelé *Geometria paradoxos*), examiné à fond, contient moins d'articles que l'on n'aurait d'abord soupçonné.

Il y a peu de géomètres, si ce n'est Euler, qui aient plus contribué de suggestions dans ce genre que l'illustre d'Alembert, et l'on se propose d'en considérer quelques-unes, une surtout qui paraît avoir beaucoup engagé son attention, vu qu'après l'avoir discutée dans un Mémoire assez connu (*Memoires* de Berlin, 1747), il y revient dans ses *Opuscules* (vol. IV, Mémoire XXIII). Cependant c'est une chose incontestable qu'il ne traite pas le sujet avec son exactitude accoutumée, paraissant plus désireux de découvrir des paradoxes que de les expliquer ou de les résoudre. Plus d'une fois, en considérant une certaine courbe, il dit, "Voilà le calcul en défaut." Ce que nous trouverons tout à l'heure n'être point dans une des matières mentionnées, et dont, dans l'autre, sa solution ne satisfait aucunement, si même elle n'est pas manifestement erronée. La courbe pourtant dont il parle mérite bien d'être pleinement examinée, et, dans ses rapports de dynamique, elle

paraît offrir plus d'un paradoxe qui avait échappé à ce grand géomètre, parce qu'il ne l'avait pas considérée mécaniquement.

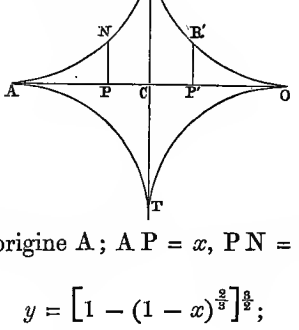
L'équation générale de la courbe est

$$y^{\frac{2}{3}} + x^{\frac{2}{3}} = a^{\frac{2}{3}};$$

en prenant $a = 1$, comme le prend d'Alembert,

$$y = \left(1 - x^{\frac{2}{3}}\right)^{\frac{3}{2}}.$$

Fig. 1.



Il prend comme l'origine A; A P = x, P N = y, A C = 1, nous donne

$$y = \left[1 - (1 - x)^{\frac{2}{3}}\right]^{\frac{3}{2}};$$

ainsi l'arc égale

$$\int \sqrt{dy^2 + dx^2} = \int dx\,(1 - x)^{-\frac{1}{3}} = -\frac{3}{2}(1 - x)^{\frac{2}{3}} + \frac{3}{2}$$

$\left(\text{la constante étant} = \frac{3}{2}\right)$; mais il suppose que l'intégrale est

$$-\frac{3}{2}\left[1 - (1 - x)^{\frac{2}{3}}\right],$$

et faisant $1 - x$ = C P, il tire

$$\text{A N} = \frac{3}{2}\left(1 - \text{C P}^{\frac{2}{3}}\right),$$

et conclut que parce que lorsque C P = 0, l'arc A R = $\frac{3}{2}$, ainsi C P étant négatif et $(-\overline{\text{C P}})^2 = +\text{C P}^2$, A R R′ devrait être égal à A N, ce qui évidemment ne peut pas être; car

$$\text{A R R}' > \text{A N},$$

et ainsi, dit-il, "Le calcul est en défaut."

Mais tout vient de ce que l'on a pris l'équation de C, et que pourtant on a pris A pour l'origine des x. Si nous prenons A comme l'origine des x et de l'équation, nous avons

$$y = \left(1 - x^{\frac{2}{3}}\right)^{\frac{3}{2}},$$

par conséquent

$$\int (dy^2 + dx^2)^{\frac{1}{2}} = \int \frac{dx}{x^{\frac{1}{3}}} = \frac{3}{2} x^{\frac{2}{3}} + C = \frac{3}{2} x^{\frac{2}{3}} + \frac{3}{2},$$

et ainsi

$$AN = \frac{3}{2} AP^{\frac{2}{3}} + \frac{3}{2},$$

et

$$ARR' = \frac{3}{3} AP'^{\frac{2}{3}} + \frac{3}{2},$$

en supposant avec d'Alembert que $CP' = CP$. Mais quand même nous prenons C pour l'origine et faisons CP positif et CP' négatif, si $CP = x$ et $PM = y$, nous trouvons

$$RR' + AR,$$

c'est-à-dire

$$ARR' > AAN.$$

Cela paraît clair et manifeste, si nous prenons l'origine qui est beaucoup plus commode que l'autre pour l'investigation des propriétés de la courbe. L'équation étant

$$y^{\frac{2}{3}} + x^{\frac{2}{3}} = a^{\frac{2}{3}} \text{ et } y = \left(a^{\frac{2}{3}} - x^{\frac{2}{3}}\right)^{\frac{3}{2}},$$

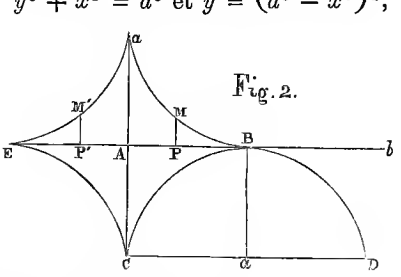


Fig. 2.

soit A le centre de la courbe :

$$AB = AE = a;$$

et prenez les valeurs positives de x entre A et E, les négatives

entre A et B. Le paradoxe supposé est que A P étant égal à A P', on trouve l'arc E M égal à l'arc E M', parce que $-\mathrm{A\,P}^2 = +\mathrm{AP}^2$. Or, voyons quel est l'arc lorsque A est l'origine; alors

$$dy = \frac{\left(a^{\frac{2}{3}} - x^{\frac{2}{3}}\right)^{\frac{1}{2}}}{x^{\frac{1}{3}}},$$

par conséquent l'arc égale

$$\int dx \frac{\left(a^{\frac{2}{3}} - x^{\frac{2}{3}} + x^{\frac{2}{3}}\right)^{\frac{1}{2}}}{x^{\frac{1}{3}}} = \frac{3}{2} a^{\frac{1}{3}} x^{\frac{2}{3}} + \mathrm{C},$$

et vu que l'arc = 0, lorsque $x = 0$, $\mathrm{C} = 0$, et $\frac{3}{2} a^{\frac{1}{3}} x^{\frac{2}{3}}$ représente l'arc. Au point E, ou lorsque $x = a$, l'arc $= \frac{3}{2} a$, au point P', mettant

$$\mathrm{A\,P'} = \frac{a}{8} \text{ et } \mathrm{A\,P} = -\frac{a}{8},$$

on a l'arc

$$\mathrm{M'}\, a = \frac{3}{8} a$$

et M A égal aussi à $\frac{3}{8} a$, à cause de l'égalité de

$$+\left(\frac{a}{8}\right)^{\frac{2}{3}} \text{ et } -\left(\frac{a}{8}\right)^{\frac{2}{3}}$$

et

$$\mathrm{E\,M'}\, a = \frac{3}{2} a = \mathrm{B\,M}\, a,$$

et enfin

$$\mathrm{E}\, a\, \mathrm{B} = 2 \,.\, \mathrm{E\,M'}\, a.$$

Ainsi nous avons

$$\mathrm{E\,M'}\, a\, \mathrm{M} = \frac{3}{2} a + 3 \frac{a}{8};$$

tandis que E M' a n'est que $\frac{3}{2} a$. Par conséquent,

$$\mathrm{E\,M'}\, a\, \mathrm{M} > \mathrm{E\,M'}\, a,$$

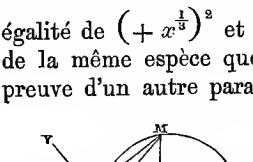
comme il doit l'être, et le paradoxe cesse. Ainsi il paraît manifeste que prenant $a = 1$, comme le fait d'Alembert dans l'équation de la courbe,*

$$M'a = Ma = \frac{3}{2}, \; M'a = Ma = \frac{3}{8}, \; EM'a = BMa = \frac{3}{2},$$

$$EaB = 3, \; Em'aM = \frac{15}{8},$$

le défaut du calcul n'existe pas.

Si pourtant on prétend encore que la branche BM ou que la section entière BMa est négative malgré l'incontestable égalité de $\left(+x^{\frac{1}{3}}\right)^2$ et $\left(-x^{\frac{1}{3}}\right)^2$, alors nous avons un argument de la même espèce que celui que soutient d'Alembert comme preuve d'un autre paradoxe allégué dans le VIII^e volume des *Opuscules* (Mém. LVIII). Il trouve difficile à comprendre comment prenant A par l'origine des x au cercle AMCB, diamètre AC = a, la valeur radicale de AM étant = $\pm\sqrt{ax}$, la négative sera AB lorsque AM est la positive, et non pas AM' dans le sens directement contraire à AM, et après avoir démontré que cette négative ne peut pas être AP, il conclut que $-\sqrt{ax}$ est AM aussi bien que $+\sqrt{ax}$. Mais il paraît véritablement que tout ce raisonnement est fondé sur erreur, et que bien qu'il ne peut pas exister un AM' parce qu'il n'y a point de cercle au-delà de A, plus qu'il ne peut y avoir de AP ; toutefois, que AM représente $-\sqrt{ax}$ autant que $+\sqrt{ax}$, et que regarder AB comme $-\sqrt{ax}$ est une erreur. Effectivement AB est trouvé, comme l'est AM, par



$$\sqrt{AP^2 + PB^2} \text{ ou } \sqrt{x^2 + y^2} = \sqrt{ax},$$

et quoique, lorsque l'on prend le diamètre pour l'axe AB = AM (d'où vient l'erreur), si toute autre ligne est prise pour

* On voit que la lettre dans la figure n'a aucun rapport avec cette lettre dans l'équation.

l'axe, A M et A B sont parfaitement inégaux, comme $a\,M < a\,B$ si $a\,\Pi\,C$ est l'axe. Cependant si le paradoxe existait du tout, il s'appliquerait autant au cas de

$$a\,M = \pm\sqrt{M\,P^2 + P\,a^2}$$

qu'au cas de

$$A\,M = \pm\sqrt{a\,x}.$$

Sa valeur négative ne serait pas, selon d'Alembert, dans la direction $a\,b$, tout directement opposée à $a\,M$, mais dans la direction $a\,B$.

On peut faire remarquer en passant que cette discussion suggère une propriété de la parabole conique dans son rapport avec le cercle, et fait voir que cette propriété n'appartient qu'à une branche de la courbe

$$A\,M = \sqrt{a\,x} \text{ et } P\,M' = A\,M,$$

si M' est dans la parabole dont le paramètre égale $A\,C = a$. Et ce rapport des deux courbes continue jusqu'à ce que x (de la parabole) $= a$, c'est-à-dire jusqu'à C' ou $y = a = C\,C'$. Ici donc nous avons la valeur négative de A M' et de P M'; P P' = P M', et ils sont directement opposés. Mais A M' et A P', comme A M et A m, ne sont pas directement opposés; chacun d'eux doit être trouvé par un procédé séparé, et l'un n'est pas le négatif de l'autre, $\pm\sqrt{a\,x + x^2}$ est la valeur de tous les deux. On voit aussi dans cette propriété de la parabole son rapport avec l'hyperbole, comme de la parabole avec le cercle, à cette différence près que ce rapport s'étend par tout le cours des deux courbes, au lieu que le rapport de la parabole avec le cercle est borné à la portion dont l'abscisse n'excède pas le paramètre. On doit de plus faire observer que même à l'époque bien antérieure de l'Encyclopédie (1754), d'Alembert avait eu des opinions particulières sur les quantités négatives (*voir* l'article *Courbes*), et sa controverse avec Euler sur les logarithmes des quantités négatives est assez connue.

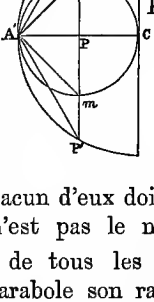


Fig. 4.

Maintenant on peut faire remarquer que quand même nous pourrions concéder l'existence du paradoxe que d'Alembert suppose sur la courbe

$$y^{\frac{2}{3}} + x^{\frac{2}{3}} = a^{\frac{2}{3}},$$

la solution qu'il donne n'est aucunement admissible. L'un des défauts du calcul, dit-il, peut être expliqué par la supposition que la branche C B (Fig. 2) est située au-delà de B, comme B D, par quoi, dit-il, il y aurait continuation de la branche a B, comme s'il croyait qu'il n'y eût aucune continuation en B C. Mais contre cette supposition s'élèvent deux objections décisives. *Premièrement*, l'équation donne aux y entre A et B des valeurs égales et opposées des deux côtés du A B, au point B, $y = 0$, et au-delà de B, comme par B d, portion de l'axe qui répond à B D, y ne peut pas exister, vu que $x = > a$, et que le radical devient $\sqrt{-1}$. Mais *secondement*, il n'y a pas possibilité qu'une courbe algébrique comme l'est celle-ci s'arrête tout court, ce que, par cette supposition, elle devrait faire au point D, tandis que la difficulté qui principalement fait recourir à l'hypothèse, la discontinuation supposée de la branche a B au point B n'est réellement, excepté que la courbe a un point de rebroussement (ou une cuspide) au point B. Si le célèbre géomètre eut examiné la courbe entiere * au lieu de se borner à une de ses portions, il aurait trouvé qu'elle est une ligne a E C B, à quatre cuspides, en rentrante en elle-même ; et il aurait certainement abandonné sa théorie et aussi sa supposition du paradoxe et du défaut du calcul. Mais c'est certain aussi qu'il aurait trouvé d'autres paradoxes que l'on doit infiniment regretter qu'il n'ait pas examinés, et dont la solution ou l'explication paraît assez difficile, pour ne pas dire impossible. Ils ont rapport avec les recherches de dynamique

* Nul doute qu'il donne la figure de la courbe entière dans la planche ; mais il ne parle du tout que des deux branches E a, a B, et sa notion que la courbe s'arrête tout court à B avait la même application à la branche E a qui devait être censée s'arrêter tout court au point a ; et il ne propose pas que cette branche E a soit continuée de l'autre côté de l'axe C a. Ainsi il paraît certain qu'il n'avait pas formé les deux branches E C, B C, et il se peut que la figure fût tracée après qu'il eut fini sa description.

plutôt qu'avec l'analyse pure, et nous nous proposons de les considérer d'abord, et de finir avec quelques autres matières touchant la courbe, indépendantes de celles renfermées dans la discussion de dynamique.

Supposons maintenant qu'un corps ou une particule fasse une révolution dans cette courbe comme orbite, le centre de la courbe étant celui de la force centripète. Cette force étant proportionnelle à $\frac{r}{2\,\mathrm{P}^3\,.\,\mathrm{R}}$ (r = rayon vecteur; P = perpendiculaire sur la tangente; R = rayon de courbure), l'on a la sous-tangente

$$\mathrm{PT} = \frac{y\,d\,x}{dy} = x^{\frac{1}{3}}\left(a^{\frac{2}{3}} - x^{\frac{2}{3}}\right),$$

la tangente

$$\mathrm{MT} = a^{\frac{1}{3}}\left(a^{\frac{2}{3}} - x^{\frac{2}{3}}\right),$$

et

$$\mathrm{P} = \mathrm{AO} = \frac{\mathrm{AT}\,.\,\mathrm{PM}}{\mathrm{MT}} = a^{\frac{1}{3}}x^{\frac{1}{3}}\left(a^{\frac{2}{3}} - x^{\frac{2}{3}}\right)^{\frac{1}{2}},$$

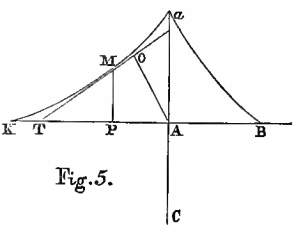


Fig. 5.

et

$$\mathrm{R} = 3\,.\,a^{\frac{1}{3}}x^{\frac{1}{3}}\left(a^{\frac{2}{3}} - x^{\frac{2}{3}}\right)^{\frac{1}{2}} = 3\,\mathrm{P};$$

par conséquent, la force centrale f est proportionnelle à

$$\frac{r}{6\,a^{\frac{4}{3}}x^{\frac{4}{3}}\left(a^{\frac{2}{3}} - x^{\frac{2}{3}}\right)^2} = \frac{\left[\left(a^{\frac{2}{3}} - x^{\frac{2}{3}}\right)^3 + x^2\right]^{\frac{1}{2}}}{6\,a^{\frac{4}{3}}x^{\frac{4}{3}}\left(a^{\frac{2}{3}} - x^{\frac{2}{3}}\right)^2},$$

ou à

$$\frac{r}{(a^2 - r^2)^2}, \text{ et si } a = 1, f = \frac{r}{(1 - r^2)^2},$$

telle est l'expression de la force en fonction de la distance

Cette force est répulsive par toute l'orbite, car P et R étant des côtés opposés de l'axe doivent avoir des signes différents, et ainsi l'expression $\frac{r}{2\,P^3 . R}$ doit être toujours négative. Mais voici un résultat de l'équation. La force devient infinie lorsque $x = 0$, c'est-à-dire au point a de l'orbite, et aussi lorsque $x = a$, c'est-à-dire au point B de l'orbite, et elle est infinie aux deux autres points E et C.

Si l'on fait le cuspide (point double) C le centre de force au lieu de A, on trouve l'expression de la force (mettant $a = 1$) comme

$$\frac{\left\{\left(x^2 + \left[1 - \left(1 - x^{\frac{2}{3}}\right)\right]^{\frac{3}{2}}\right)^2\right\}^{\frac{1}{2}}}{2\,x^{\frac{4}{3}}\left[1 - \left(1 - x^{\frac{2}{3}}\right)^{\frac{3}{2}} - x^{\frac{2}{3}}\left(1 - x^{\frac{2}{3}}\right)^{\frac{1}{2}}\right]^3 \left(1 - x^{\frac{2}{3}}\right)^{\frac{1}{2}}};$$

et ici comme dans l'autre cas, la valeur de la force est infinie pour les deux valeurs de x, $x = 1$ et $x = 0$, et qui est assez remarquable ; elle devient infinie au point B dans la portion de l'orbite C B où la force est attractive aussi bien que dans la portion a B où elle est répulsive, ou dans toutes les quatre branches lorsque C, au lieu de A, est le centre de force. Même résultat si l'on prend comme centre de force les points E et B. Ainsi il est manifeste que dans tous les cas la valeur de la force devient infinie lorsque le mobile arrive à l'un des points de rebroussement.

Avant de discuter ce résultat, il sera bon de faire observer que la même chose arrive dans le cas des autres orbites, et que toutes les difficultés que l'on éprouve dans la courbe dont nous sommes occupés se recontrent dans ces autres trajectoires. Par exemple dans la lemniscate

$$y = x\,(1 - x^2)^{\frac{1}{2}},$$

dont la sous-tangente est

$$\frac{x\,(1 - x^2)}{1 - 2\,x^2},\quad P = \frac{2\,x - 3\,x^3}{(2 + 4\,x^4 - 5\,x^2)^{\frac{1}{2}}} \text{ et } R = \frac{(2 - 4\,x^4 - 3\,x^2)^{\frac{3}{2}}}{2\,x^3 - 3\,x};$$

ainsi

$$f = \frac{(2x^2 - 3)\,(2 - x^2)^{\frac{1}{2}}}{x\,(2 - 3x^2)^3};$$

par conséquent, f est infini soit que $x = 0$, soit que $x = \sqrt{\frac{2}{3}}$. Mais l'analogie avec notre courbe paraît plus complète si l'on prend le centre de force à l'une des extrémités de l'axe ; car alors le mobile tournant dans l'orbite passe par le milieu de l'axe, d'un côté à l'autre de cet axe, et à ce point la force est infinie. Même chose dans la ligne que Newton appelle *Parabola nodata* (*Enumeratio Lin. tertii ordinis*, IV. 13). Il n'en donne pas l'équation, mais on peut la déduire de l'équation générale ; elle est

$$y = x\,(a - x)^{\frac{1}{2}},$$

qui nous donne pour la sous-tangente $\frac{2x\,(a - x)}{2a - 3x}$,

pour la perpendiculaire $\frac{(4ax - 5x^2)\,(a - x)^{\frac{1}{2}}}{[(2a - 3x)^2 + 4\,(a - x)]^{\frac{1}{2}}}$,

rayon de courbure $\frac{-[(2a - 3x)^2 + 4\,(a - x)]^{\frac{3}{2}}}{2x}$,

et r étant égal à $x\sqrt{a + 1 - x}$, nous avons

$$f = \frac{2\,(a + 1 - x)^{\frac{1}{2}}}{x\,(4a - 5x)^3\,(a - x)^{\frac{3}{2}}}.$$

La lemniscate a, comme on sait, la figure d'un huit de chiffre. La *parabola nodata* se compose d'un ovale et deux branches infinies, sans asymptotes.

Il y a deux difficultés qui principalement se présentent dans cette discussion. La *première* est la transition du corps mobile de l'une des branches de notre courbe à l'autre, une discontinuité complète existant à ce que l'on a souvent prétendu.

La *seconde* difficulté est la valeur infinie de l'expression pour la force à certains points de l'orbite. Sur la première de ces difficultés, et en partie sur la seconde aussi, la considération de la *parabola nodata* et des courbes de cette forme paraît répandre de la lumière. Car si l'on prend pour centre de force un point de l'axe, A', hors de l'ovale, la force répulsive fera passer le mobile de *a* par B, *m* (Fig. 6) jusqu'au point A où cette force devient attractive; et en changeant de position de l'un des côtés de l'axe à l'autre, le corps passe par A, où la force devient infinie. Or on peut supposer que la ligne A B, l'axe de l'ovale, décroît indéfiniment jusqu'à ce qu'elle s'évanouit; et alors, comme l'a remarqué Newton lui-même, l'ovale devient une cuspide (point de rebroussement). Ainsi cela pourra arriver dans le cas de chacune des quatre cuspides de notre courbe. Toutes ont pu être des ovales dont les axes s'étaient évanouis; mais à l'instant d'évanouissement de l'axe, et lorsque l'ovale fut presque éteint et réduit aux dimensions les plus petites, pour ne pas dire infinitésimales, le corps avait été poussé par la force d'abord répulsive, puis à l'extrémité de l'axe de l'ovale attractive, et la valeur infinie de la force avait existé au point A réuni au point B après, l'extinction de cette force ayant été infinie à tous ces deux points avant l'extinction de l'ovale.

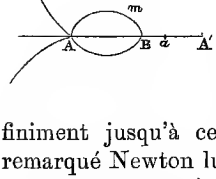


Fig. 6.

Sur la *seconde* difficulté, il y a un exemple plus familier dans le cas du cercle, lorsqu'il est l'orbite d'un mobile, et que le centre de force est dans la circonférence; car alors cette force devient infinie $\left(\text{l'expression était } \frac{1}{r^5} \text{ au lieu de } \frac{1}{r^2}\right)$ au passage du corps par le centre: or $r = 0$; mais à l'autre extrémité du diamètre elle ne l'est pas comme elle est dans la *parabola nodata*.

Un ami très-savant dans la géométrie avait pensé que l'explication de l'infini au passage du corps de l'un à l'autre côté de l'axe se trouve dans ce que la force finie ne peut

aucunement le faire passer d'une branche de la courbe, et qu'il doit s'éloigner à l'infini, plutôt que de prendre l'autre branche ; mais l'exemple de la lemniscate paraît repousser cette notion, aussi bien que celui de la *parabola nodata*, et même du cercle ; car dans tous ces cas, le corps continue son mouvement sans aucune interruption en passant par le point où la force devient infinie.

L'analogie des forces qui agissent en raison inverse de la distance vient nous frapper dans cette discussion. On peut pourtant remarquer que lorsque la gravitation est supposée d'agir avec une force infinie, vu que la distance n'existe plus, il est question du centre du globe, où toute la masse est supposée réunie, et aussi il y a toujours le rayon du globe entre le corps qui gravite et le centre de force. Que devrait-on dire de la force magnétique, soit que cette force est, comme l'a supposée Newton, l'inverse cube de la distance, soit l'inverse carré comme l'on pense aujourd'hui ? Dans l'un ou l'autre cas au point de contact la force devient infinie, et pourtant les phénomènes ne nous déclarent aucune force infinie. Même remarque peut se faire sur toute force ou influence quelconque venant d'un centre et propagée à la circonférence, de force ou d'influence. Peut-être faut-il admettre la théorie de Boscowich, qui suppose une force répulsive plus près des corps, et croissant en raison inverse de la distance, et ainsi contrebalançant ou remplaçant la force d'attraction ; et les spéculations sur l'impossibilité d'un contact complet ont du rapport avec la proposition de l'infini, en tant que l'on pourrait déduire cette impossibilité de la non-existence dans la nature d'une force distrayante (divellante).

Mais il y a une plus grande difficulté que celle que nous avons considérée dans l'expression de l'infini. Les cas que nous venons de considérer ont rapport avec des points de l'orbite, là où elle passe d'un côté de l'axe à l'autre et que la tangente devient nulle ou infinie. Mais que dire d'une valeur infinie aux autres points, comme dans la lemniscate au point où $x = \sqrt{\frac{2}{3}}\, a$, et dans la *parabola nodata*, à $x = \frac{4}{5}\, a$? Ce-

pendant ce n'est pas à ces valeurs de x que les courbes sont le plus éloignées de l'axe et que leurs tangentes sont infinies; au contraire, c'est là où $x = \frac{1}{2}a$ dans la lemniscate, ou au milieu de l'axe de l'ovale, et là où $x = \frac{2}{3}a$ dans la *parabola nodata*. Si l'on n'était pas assuré que le procédé pour obtenir la valeur de la force centrale est de toute exactitude, par la conformité de ses résultats aux lois les plus connues de dynamique, particulièrement à la raison inverse de la distance des foyers des sections coniques, on serait tenté de soupçonner quelque paralogisme en observant le résultat des mêmes procédés dans le cas que l'on vient de traiter. Pourtant, au lieu de dire paradoxe avec l'illustre géomètre dont nous avons osé tant parler, il vaut mieux de soupçonner quelque erreur dans l'application des procédés du calcul, quelque confusion telle que l'on peut remarquer dans ses raisonnements, confusion, c'est-à-dire des valeurs algébriques et géométriques, à ce qui regarde le signe négatif, et ainsi cela sera non pas le calcul en défaut, mais ceux qui l'appliquent.

Les propriétés générales et géométriques de la courbe qui nous a occupé d'un autre point de vue, sont assez curieuses pour mériter une discussion plus suivie.

1. Ce qui nous frappe d'abord, c'est l'exception que paraît ajouter cette courbe aux autres exceptions au célèbre lemma (XXVIII) de Newton, portant qu'aucun ovale n'est susceptible ni de quadrature ni de rectification. D'Alembert a noté sa rectification, qui ne peut pas être douteuse, vu que

$$\sqrt{dy^2 + dx^2} = \frac{a^{\frac{1}{3}}\,dx}{x^{\frac{1}{3}}},$$

dont l'intégrale est

$$\frac{3}{2}a^{\frac{1}{3}}x^{\frac{2}{3}} + C;$$

et vu que $x = 0$, l'arc $= 0$; ainsi $C = 0$. Mais la quadrature aussi est possible; car

$$\int y\,dx = \int dx\left(a^{\frac{2}{3}} - x^{\frac{2}{3}}\right)^{\frac{3}{2}},$$

ou (si nous mettons $x = z^3$)

$$= \int 3z^3\,dz\,\frac{\left(a^{\frac{2}{3}} - z^2\right)^2}{\left(a^{\frac{2}{3}} - z^2\right)^{\frac{1}{2}}},$$

et l'aire

$$3a^2\left[\frac{1}{16}\sqrt[3]{\frac{x}{a}}\cdot\sin^{-1}+\left(\frac{x}{a}\right)^{\frac{5}{3}} - \frac{7}{24}\frac{x}{a} + \frac{1}{16}\sqrt[3]{\frac{x}{a}}\times\right.$$

$$\left.\sqrt{\left(1-\frac{x}{a}\right)^{\frac{2}{3}}}\right] + C;$$

et C = 0 si l'on prend l'aire depuis A a; et l'aire entière A a B, x étant $= a$, est $\frac{3}{32}\,\Pi\,.\,a^2$.

On dira peut-être que lorsque Newton a énoncé l'impossibilité, il s'est servi de l'expression *figura ovalis*, et qu'il a pu vouloir se borner aux courbes d'une courbure continue, comme le cercle et l'ellipse. Pourtant l'opinion universelle porte qu'il avait regard à toute courbe rentrant en elle-même ; et cette opinion est appuyée par la considération qu'en donnant les cas d'exception à son proposition, il se borne aux cas des courbes qui ont un arc infini avec leur ovale. Mais aussi il est certain que la démonstration de sa proposition s'applique aux courbes telles que celle qui nous occupe à présent. Car on peut prendre le centre pour le pivot sur lequel tourne la règle qui est supposée. Encore on n'a jamais prétendu que la lemniscate fût exclue de la proposition, toute carrable qu'elle soit, quoique non rectifiable.

2. La courbe est une épicycloïde engendrée par le roulement d'un cercle dont le diamètre est un quart du diamètre du cercle extérieur. Si le rayon de ce cercle $= a$, l'équation de la courbe étant

$$y^{\frac{2}{3}} + x^{\frac{2}{3}} = a^{\frac{2}{3}},$$

le rayon du cercle roulant est $\frac{a}{4}$.

3. Si l'on décrit une ellipse sur l'axe de la courbe $y^{\frac{2}{3}} + x^{\frac{2}{3}} = a^{\frac{2}{3}}$, et que la somme des axes de l'ellipse $= a$, elle touchera la courbe.

4. La courbe a quelque ressemblance avec la développée de l'ellipse; mais elle ne l'est pas; car l'équation de cette développée diffère de notre équation. Elle est

$$y^{\frac{2}{3}} + a^{\frac{2}{3}}\, x^{\frac{2}{3}} = (1 - a^2)^{\frac{2}{3}},$$

les axes de l'ellipse étant 1 et a. Mon savant ami M. Routh a examiné la question, n'ayant doute que notre équation ne soit celle de quelque développée; et il trouve que dans un cas $y^{\frac{2}{3}} + a^{\frac{2}{3}}\, x^{\frac{2}{3}} = a^{\frac{2}{3}}$ est la développée d'une ellipse, notamment de celle dont l'équation est

$$y^2 + \frac{x^2}{a^2} = \frac{1}{(1 - a^2)^2}.$$

Lorsque $a > 1$ ou < 1, la courbe est la développée de quelque ellipse. Mais dans les cas qu'elle ne le soit pas, elle est fréquemment la développée d'un ovale de quelque espèce différente de l'ellipse. Lorsque $a = 1$, le procédé manque complétement, et l'on ne peut avoir aucune développée. Dans plusieurs livres élémentaires, on remarque la développée de l'ellipse représentée sous la forme de notre courbe; mais elle est complétement différente dans le fond.

5. La perpendiculaire à la tangente du centre de la courbe (a étant $= 1$) est $x^{\frac{1}{2}} \left(1 - x^{\frac{2}{3}}\right)^{\frac{1}{2}}$ et le rayon de courbure $3 \,.\, x^{\frac{1}{3}} \left(1 - x^{\frac{2}{3}}\right)^{1}$. Ainsi $\mathrm{R} = 3\,\mathrm{P}$.

6. Si la tangente est prolongée jusqu'à ce qu'elle rencontre les axes perpendiculaires de la courbe, cette tangente ainsi prolongée est toujours égale à l'axe, c'est-à-dire à a.

7. De cette propriété de la tangente prolongée constante, résultent des conséquences assez remarquables. Entre autres on peut noter celle-ci : Si un point est poussé sur une ligne donnée entre deux perpendiculaires, avec une vitesse uniforme, tandis que cette ligne est poussée sur l'une des deux perpendiculaires avec une vitesse inversement proportionnelle à la dis-

tance de son entrémité de la perpendiculaire, le point mouvant décrit la courbe $y^{\frac{2}{3}} + x^{\frac{2}{3}} + a^{\frac{2}{3}}$, les axes étant chacun = a.

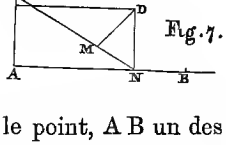


Fig. 7.

Soit E N la ligne, M le point, A B un des axes. Si le mouvement de M sur E N est uniforme, et que N est poussé avec la vélocité $\frac{1}{A N}$, M décrit la courbe. Encore prenez D pour le centre instantané de rotation de E N; la perpendiculaire D M, de D sur E N, coupe E N en M, qui est dans la courbe; le mouvement de rotation de la ligne était combiné avec le mouvement en ligne directe du point.* Si le point M reste sans mouvement sur E N, tandis que E N est poussée sur A B et A C, M décrit une ellipse, que devient un cercle si M est au milieu de E N.

8. La propriété de la tangente prolongée constante mène naturellement à la comparaison de notre courbe avec une autre que j'avais décrite il y a soixante ans dans les *Phil. Trans.* (1798, part. II), comme ayant une tangente constante, et par conséquent la sous-tangente†

$$y \frac{dx}{dy} = \sqrt{a^2 - y^2},$$

a étant la longueur de la tangente. L'équation différentielle

$$dx = \frac{dy}{y} \sqrt{a^2 - y^2},$$

nous donne pour intégrale

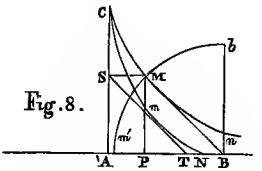
$$x = \pm \sqrt{a^2 - y^2} + a \,.\, \log. \left(\frac{y}{a \pm \sqrt{a^2 - y^2}} \right).$$

* Cette proposition s'est presentée à mon illustre confrère M. Chasles, qui a eu la bonté de me la communiquer.

† Voir Art. 1 de ce volume.

Et la courbe est de la formule (fig. 8) C M n, ayant une cuspide à C, et étant convexe à l'axe A B ; notre courbe aux quatre cuspides est C m N, ayant la tangente prolongée S T constante, = A C = A B ; tandis que la logarithmique C M n a la tangente M B = A C, et a = A C.

Fig. 8.



L'arc de celle-ci S $\frac{a\,dy}{y}$ = a . log. y + C, et comme C . M n = y et x' = 0, C = 0 l'aire = $\surd . y\,dx = s\,dy\surd\overline{a^2 - y^2}$, ainsi la courbe a ce rapport avec le cercle, que M m' etant un cercle dont le rayon = B M = a, l'aire de la courbe, A C M P est égale à l'aire du cercle P M b B.

Ce rapport avec le cercle n'existe par dans l'autre courbe C m N ; non plus que cette autre propriété de la logarithmique, qui la lie avec la tractrice de la ligne droite.*

* This tract is the Mem. read June 1857, before the National Institute. The volume of Mem. is not yet published, but only the *Compte Rendu*.

VI.

ARCHITECTURE OF CELLS OF BEES.*

Quoique peu de sujets aient occupé davantage les naturalistes de tous les siècles, et même les géomètres depuis le temps d'Aristote et de Pappus, que l'abeille, ses habitudes, et son architecture, on ne peut pas nier qu'avec un grand progrès et des vérités importantes, des erreurs ne se soient glissées assez remarquables pour mériter une explication. Aussi est-il certain qu'un peu d'attention suffit pour dissiper les erreurs que la négligence ou les préjugés ont fait naître chez les géomètres également, et chez les naturalistes, tandis que tous les deux s'étant arrêtés tout court ont manqué faire des observations intéressantes qui se présentent en relevant les erreurs. De ces erreurs l'une est entomologique, l'autre géométrique. L'avancement de nos connaissances sur ce sujet est d'un intérêt, et même d'une importance sous plus d'un point de vue, qui justifie quelques détails.

I. Dans les transactions de la Société Wernerienne (vol. II, p. 260), le Dr. Barclay, célèbre anatomiste d'Edimbourg, a annoncé une découverte que les naturalistes ont cité l'un après l'autre, comme constatée sans en examiner les preuves; ou peut-être trompés par les mêmes apparences qui avaient égaré M. le docteur. Il se peut qu'ils furent disposés de l'accepter d'autant plus que nous devons à un autre anatomiste, un grand physiologiste (le célèbre J. Hunter), la plus im-

* This tract is the memoir read May, 1858, before the National Institute, "Recherches Analytiques et Expérimentales." The volume of Mem. has not yet been published, but only the *Compte Rendu.*

portante des découvertes en cette branche de science. La proposition dont il s'agit porte que chaque alvéole, tant pour ses parois hexagones que pour son fond ou base pyramidale, est double, de manière qu'elle est séparée et indépendante des alvéoles qui l'entourent, et formée d'elle-même; que ses parois de cire sont attachés aux parois des autres alvéoles par une substance agglutinante; et que si cette substance est détruite, chaque alvéole peut être entièrement séparée des autres. Le Dr. Barclay prétend aussi que les alvéoles des guêpes sont doubles, et que leur substance agglutinante est plus facilement détruite que ne l'est celle des alvéoles d'abeilles.

Il paraît presque impossible de croire à cette structure après les observations des naturalistes, surtout de Réaumur et de Huber, sur la manière dont l'abeille travaille. Elle ne peut pas s'insinuer entre les deux plaques de cire pour les polir; car elles sont parfaitement et également polies. La substance agglutinante n'existe pas dans la cire. Mais avant tout, l'inspection des gâteaux de cire prouve que si les alvéoles n'ont jamais servi pour faire éclore des œufs, et pour l'éducation des vers et des chrysalides, on ne voit aucune trace de parois doubles. Celles dans lesquelles les larves ont été transformées en chrysalides présentent l'apparence qui a égaré le Dr. B.; et l'on remarque que son Mémoire était accompagné d'un gâteau de cire vieille, dont les alvéoles avaient entretenu plusieurs successions d'insectes. Mais venons aux phénonèmes de plus près.

Un gâteau fut choisi dont une portion n'avait jamais servi ni pour amasser, ni pour engendrer, et dont l'autre portion avait reçu une seule couvée. La première portion était parfaitement blanche; la seconde avait une légère teinte jaunâtre, ou une nuance brune très-légère; et dans plusieurs endroits, on voyait de ces raies rouges, observées par Huber, et qu'il prouve être une matière végétale cueillie des arbres, et surtout du peuplier. Le gâteau avait été fait au mois d'août, et fut pris quatre semaines plus tard. Etant plongé dans l'alcohol, peu ou point de changement fut produit avant que l'alcohol fût échauffé; et alors la cire s'est fondue tout de suite; la partie blanche fut

entièrement dissoute, sans qu'aucune trace des alvéoles restât; et la partie jaunâtre ne se fondit pas entièrement. De cette partie il decoula de la cire fondue, mais le gâteau gardait sa forme et ses dimensions à peu près, bien que la chaleur continuât. Lorsque l'alcohol bouillait, cette portion du gâteau dans laquelle les abeilles avaient été produites se séparait en morceaux, mais il fallait remuer pour aider la séparation, et pour faire fondre toute la cire. Lorsqu'un gâteau plus vieux, et qui également avait produit des abeilles, fut mis dans l'alcohol moins bouillant, la séparation et la fonte de la cire demandèrent plus de temps; mais enfin en le remuant toute la cire se fondait, excepter cette petite portion que l'alcohol ne prend pas, et qui restait dans la forme de petites globules; mais toutes les alvéoles sont restées dans leur forme, chacune séparée des autres, et pas une seule ne fut composée de cire, mais toutes de soie, de cette soie, c'est-à-dire, que forme le ver avant sa transmutation en nymphe ou chrysalide, et dont il tapisse l'intérieure de l'alvéole de cire. Avec de l'eau bouillante on peut opérer de même, mais plus lentement. Avec l'esprit de térébinthe, la fonte de la cire est très-rapide, seulement on ne peut pas voir par cette forme de l'expérience dans quelle partie de l'alvéole la cire existe. L'acide sulfurique peut faire précipiter ou fondre la cire sans la dissoudre autrement qu'en très-petite quantité, et les alvéoles restent. L'expérience fut répétée avec un gâteau dans lequel plusieurs couvées avaient été produites. Les alvéoles furent moins larges, leurs parois plus épaisses, et leur couleur, une nuance brune foncée, çà et là presque noire.

Maintenant, examinons les alvéoles séparées par ce procédé. Chacune fut formée d'un prisme hexagone terminé par un pyramide de trois rhombes égaux; en un mot, chacune fut exactement à la matière près une alvéole comme celles de cire; mais formée de matériaux entièrement différents. Les parois et la base furent composés d'une pellicule extrêmement mince et semi-transparente qui ressemblait à la feuille de battant d'or, mais absolument sans ride. Les plus vieilles gardèrent la forme plus exactement; de sorte que leurs angles et leurs plans furent aussi bien définies que le sont ceux de cire dans le gâteau neuf.

Mais ce n'était point là une seule pellicule, comme celles qui n'avaient servi qu'à un seul ver ou insecte; au contraire, ces alvéoles avaient plus d'une pellicule, l'une du dedans de l'autre; et ces pellicules pouvaient être séparées au nombre de cinq ou six, toutes provenantes de la même alvéole, et toutes gardant les formes hexagones et rhomboïdales; mais la sixième avait des rhombes beaucoup moins marqués; et s'il fut jusqu'à une neuvième ou dixième, la base devenait plutôt sphérique que pyramidale, et était très-peu profonde. Les parois hexagones de toutes les alvéoles gardaient cette forme; seulement les derniers (c'est-à-dire les intérieurs) avaient un plus petit diamètre. Dans les angles il y avait un peu de la matière rouge, mais beaucoup plus dans le fond, ou partie pyramidale. Cette base dans les alvéoles internes paraissait presque remplie de rouge. La bouche de l'hexagone a toujours un bord composé principalement de cette matière.

J'ai trouvé impossible de dissoudre, ou de quelque manière que ce fût d'affecter la pellicule, soit en la macérant dans l'alcohol, dans l'esprit de térébinthe, ou de tout autre réactif, même bouillant, excepter que la matière rouge après une longue macération était déposítée, et donnait un teint jaunâtre à la liqueur.

L'exactitude avec laquelle la pellicule tapisse la cire de l'alvéole est très-remarquable. Il n'y a pas le moindre ride, ni intervalle. Tout est couvert, et avec une pellicule de la même épaisseur partout, exceptez que la matière rouge aux angles fait varier un peu l'épaisseur de la pellicule à ces angles. Tout l'intérieur de l'alvéole forme un tapis uni, sans aucune couture, et sans aucun ciment. Car après avoir soupçonné que la matière rouge aux angles pourrait servir de ciment, cette notion a été de suite contredite par l'inspection de ces parties angulaires qui n'avaient jamais eu de couche de rouge, et de celles dont la matière rouge avait été grattée et enlevée. Aussi j'ai trouvé que la matière rouge était exactement sur les mêmes portions de la pellicule. Car en découpant un hexagone contenant plusieurs pellicules, de manière à étendre tous les six côtés (comme à la figure 1), on voyait que cette matière était repartie

dans toutes ces pellicules de la même façon. La fig. 2 fait voir la distribution dans les angles de la base ; et là il n'y a pas de



Fig. 1.

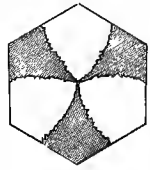


Fig. 2.

différence entre les pellicules successives par rapport à cette matière, excepter que, étant rétrécie dans celles qui sont le plus éloignées de la cire, la matière rouge occupe une plus grande proportion, et la partie sans rouge une plus petite, la somme totale du rouge étant le même dans toutes les pellicules. Une pellicule de la même substance, aussi transparente mais bien plus épaisse, tapisse l'alvéole de la reine abeille. La matière rouge est plus également répandue sur sa surface en nuages et raies, vu qu'il n'y a point d'angles qu'elle doit doubler. La pellicule de cette alvéole royale prend la forme de la cire ; mais ce qui est très-remarquable, c'est qu'elle n'est pas toujours sur l'intérieur de la cire. Quelquefois elle est enfermée dans la cire, dont une couche est même plus épaisse que les parois de cire, et j'en ai examiné qui avait une épaisseur beaucoup plus grande. On peut constater que dans les alvéoles ordinaires, la cire n'est pas plâtrée sur la pellicule. On a examiné de près les plus vieux gâteaux ; et jamais l'on n'a trouvé un seul exemple de pellicule entre deux couches de cire, excepté dans l'alvéole royale. Aussi on a vu clairement que dans les plus vieux gâteaux, qui donnent plus de neuf ou dix suites de pellicules dans les alvéoles ordinaires, l'alvéole de la reine seule n'avait qu'une pellicule.

La manière de former ces pellicules et de tapisser l'alvéole mérite beaucoup plus d'attention qu'elle n'a jusqu'à présent reçue. L'opinion générale paraît être qu'elles sont fabriquées en tissue. M. Daubenton (Encyclop. 1751, vol. I, p. 21) décrit le procédé de tisser comme opéré en mettant des fils très-fins et très-près, l'un de l'autre, qui se croisent. Huber semble être de cette

opinion, et que le ver tapisse à la fois qu'il forme la toile, et non pas qu'il fait la toile d'abord et puis l'applique aux parois. Il paraît presque impossible de croire que la toile est faite par cette opération en même temps qu'elle est appliquée. Car la largeur de l'alvéole varie dès le commencement de la partie pyramidale à chaque point, et bien que le ver n'eût à tisser qu'autour de la même circonférence et sans avoir le moindre aide pour le régler, cependant il devrait faire la toile si exactement adaptée à la circonférence, qu'en l'appliquant il n'y en aurait ni de trop, ni de trop peu, et sans aucune ride. C'est certain qu'une telle opération surpasse infiniment tout ce que fait jamais l'insecte parfait. Avec toutes les resources de notre science et de notre mécanique, on peut affirmer hardiment qu'il nous serait impossible de tisser un sac de toile de largeur variante à tous les points, et pourtant si exacte dans ses proportions qu'étant découpé ou fendu, il tapisserait les murs sans la moindre ride, et sans aucune intervalle.* La difficulté est moins grande si le ver tapisse au moment d'appliquer, et qu'à chaque instant il place la toile qu'il vient de fabriquer. Mais c'est plus probable qu'il n'y a pas de tissage du tout. Certainement la plus puissante loupe ne fait voir aucune filature. Apparemment une matière glutineuse est répandue par le ver sur les parois; et toute difficile que soit cette opération aussi, elle l'est beaucoup moindre que l'autre, vu que le ver a les parois pour le guider. Il n'est pas douteux pourtant que le résultat soit extraordinaire ; car non-seulement il y a une égale épaisseur par toute la pellicule, mais le ver en

* J'ai mesuré et calculé la différence de la surface des trois portions du tuyau de l'alvéole. La partie pyramidale, la partie voisine, composée d'une portion de la pyramide et d'une partie de l'hexagone, et la partie de l'hexagone seule. En supposant toutes les trois portions de la même hauteur, les surfaces sont comme 3·03, 5·05, et 4·04 (lignes carrées) respectivement. Ainsi en filant le tissu le ver devrait tisser exactement dans ces proportions; et en filant les deux premières parties il devrait changer à chaque instant la vitesse de son travail, vu que le contour, ou circonférence de la surface ne reste par la même, mais change à chaque instant, et que le ver devrait tisser en suivant cette circonférence. Cette circonférence varie depuis le fond pyramidale de zéro à douze lignes sur la surface ci-dessus notée.

faisant le contour pour plâtrer, doit s'arrêter exactement au point d'où il est parti, vu qu'il n'y a pas le moindre vestige de la jonction des deux côtés ; pas la plus petite différence d'épaisseur.

Il paraît presque certain que la pellicule est douée de différentes qualités, selon qu'elle est nouvellement faite ou le contraire. On expliquerait difficilement le phénomène des vers la fabriquant toujours avant de devenir chrysalides. Car le premier ver avait déjà tapissé la cire ; et s'il n'avait besoin que de se protéger lui-même, ou sa chrysalide de la cire, le second ver qui naîtrait dans la même alvéole serait protégé par la même pellicule ; et ainsi des neuf ou dix autres successeurs ; et pourtant tous doivent faire une pellicule chacun, même en diminuant l'espace, et à la fin presque la remplissant. Il ne nous est aucunement permis de dire que voici une des méprises que fait l'instinct quelquefois, parce que ces méprises sont toujours accidentelles ; par exemple, lorsque la mouche trompée par l'odeur d'un fleur et croyant que c'est de la charogne, y pond ses œufs. Mais chez les abeilles c'est une méprise continuelle et régulière, s'il en est une ; car elles préfèrent toujours déposer les œufs dans une alvéole où une couvée a été élevée, et où par conséquent il y a une ou plusieurs pellicules de laissées aussi parfaites que pourraient être une pellicule nouvellement faite. L'instinct de l'insecte étant surtout d'économiser des matériaux et du travail, il le porte d'abord à préférer le vieux gâteau pour ne pas faire des alvéoles de la cire vierge ; mais comment alors le même instinct ne le porterait-il pas à profiter des pellicules qu'il trouve dans les alvéoles ? Mais au lieu de cela le ver prodigue son matériel et son travail à faire une pellicule neuve pour lui-même et pour sa chrysalide. Un instinct qui manque aussi souvent qu'il réussit ne peut aucunement être comparé à ces méprises ou fautes accidentelles. Ainsi il paraît impossible de douter que la pellicule fraîche nouvellement faite possède quelque qualité nécessaire pour l'entretien de la chrysalide.

Ceux que les alvéoles de soie des abeilles avaient égaré jusqu'à croire que les parois de cire sont doubles, sont tombés

dans la même erreur à propos de la structure des guêpes. Ils font observer même que la duplication est plus facile à voir dans le gâteau guêpe que dans le gâteau abeille, à cause disent-ils que la matière agglutinante est moins adhérente. J'ai soigneusement examiné ces structures, et il n'y a pas le moindre doute que l'alvéole brune, faite de la limaille de bois, est doublée d'un papier blanc, très-fin, soit filé, soit plâtré; et on peut le séparer facilement lorsqu'il reste humide, mais aussi quoique plus difficilement si on ne peut jamais fendre le parois de manière à en faire deux de mêmes matériaux. Si on le tranche ou fende au milieu, on trouve d'un côté une plaque brune, de l'autre une plaque brune d'un côté et blanche de l'autre, nommément le côté double du papier blanc. La guêpe étant beaucoup moins économe des matériaux de sa construction que l'abeille, vu qu'ils sont plus facile à trouver que n'est la cire à produire, n'économise que l'espace et le travail en formant l'alvéole brune. Les parois donc peuvent être construites par le mélange de la limaille de bois agglutinée avec quelque liquide savetée par l'insecte lui-même. Mais la pellicule blanche est évidemment une sécrétion entièrement, soit par le ver en devenant chrysalide, soit par la guêpe elle-même avant de pondre l'œuf qui produit le ver. Ce papier est très-fin; il est demi-transparent, et on a trouvé qu'il est capable de recevoir l'encre sans barbouiller, comme s'il eût été collé ou lavé exprès. On sait combien les guêpes ont anticipé depuis vingt siècles nos fabricants de papier; mais pour papier blanc et lavé, je ne l'avais jamais entendu dire.

II. Les erreurs qu'on vient de marquer, et qui ont conduit en les exposant à des nouvelles observations sur l'économie de l'insecte, ont été soutenues, et en partie anticipées par des autres erreurs dont l'origine fut le désir de chasser les doctrines établies depuis bien longtemps sur la merveilleuse opération de l'instinct de l'insecte. Plusieurs philosophes ont prétendu démontrer, les uns que l'abeille n'est pas la véritable architecte des alvéoles qui sont produites, disent-ils machinalement par les propriétés et les mouvements de la matière; les autres que

l'insecte aurait pu travailler bien plus artistement. Ces erreurs, qui proviennent de géométrie mal entendue, autant que de négligence dans les observations sur l'insecte, bien examinées nous conduisent à la conclusion, non pas seulement qu'il n'y a aucun fondement pour les objections élevées, mais que les opérations de l'insect sont encore plus étonnantes que l'on avait ci-devant supposé.

La théorie de Buffon paraît la plus insoutenable, pour ne pas dire la plus absurde, égarement; et ceux qui se rappellent le controverse qu'il avait, malheureusement pour sa réputation, engagé contre Clairaut, verront encore une preuve que le grand historiographe des animaux aurait bien fait de ne toucher jamais le domaine du géomètre. Ayant cru percevoir des hexagones dans les boules de savon (ce qui n'est qu'une illusion optique occasionnée par les lignes de contour qui se croisent, sans qu'il y ait un seul hexagone de forme), il suppose que la cire étant d'abord disposée en cylindres, ces cylindres par leur pression mutuelle s'applatissent et forment des tuyaux hexagones. Mais pour ne rien dire sur l'omission totale d'explication de la base pyramidale, même la théorie ne prouve aucunement la formation hexagone, vu qu'aucun cylindre n'a jamais existé, Huber ayant prouvé que l'abeille travaille d'une toute autre manière; et puis si l'on suppose toute la cire formée en cylindres, la pression manque qui est le fondement de l'hypothèse. Supposons même que la gravité de la partie supérieure du gâteau la fait presser sur la partie inférieure, les alvéoles seront dans toutes les parties de grandeur différente, contrairement aux phénomènes; et qu'arriverait-il si le gâteau fut formé horizontale et non pas verticale? Alors point de pression; et pourtant les alvéoles dans ce cas-là ont exactement la même figure. On ne doit pas s'étonner que Daubenton, dans son admirable article dans l'Encyclopédie cité plus haut, ne fasse aucune mention de la théorie de son maître et patron, avec qui il n'avait pas encore à cette époque eu les différends qui seuls ont terni la mémoire de Buffon, pour son traitement de cet éminent savant et admirable homme. Mais une erreur d'une autre espèce a été commise par des auteurs, tous de quelque

réputation comme géomètres, et dont l'un fut même assez distingué, des auteurs bien au-dessus de Buffon dans les sciences sévères. Nous commencons par celui du plus grand mérite, et qui jusqu'à présent à été censé d'avoir raison, son erreur ayant été rejetée sur les observations supposées défectueuses d'un autre et très-célèbre philosophe.

Le grand pas qu'avaient fait les connaissances sur l'architecture de l'abeille depuis les observations de Pappus sur la forme hexagone, était l'examination de la base ; et le fameux Maraldi avait trouvé que pour que ces bases s'accommodassent sans perdre de l'espace, elles devaient être toutes rhomboïdales, formées de trois rhombes égaux. Puis il a mesuré les angles de ces rhombes ; et il trouva que l'un était de 109° 28′, l'autre de 70° 32′.* La raison de cette proportion a échappé à ce géomètre distingué et naturaliste encore plus éminent. Mais plus tard Réaumur, avec sa sagacité si connue, a soupçonné que la proportion observée par son prédécesseur devait être celle qui donnait dans la construction de l'alvéole le *minimum* de travail et de matériel ; et il proposait à M. Koenig (digne élève des Bernoullis) le problème de déterminer les angles du rhombe qui couperont le prisme hexagone de manière à former la figure composée d'une pyramide, et des portions triangulaires du hexagone, avec le minimum de surface. M. Koenig, ne sachant pas la mesure de Maraldi, ni même la conjecture de Réaumur, donna sa solution, et faisait les angles de 109° 26′ et 70° 34′. Lorsqu'il a appris la théorie de Réaumur et la mesure de Maraldi, il croyait comme Réaumur et tous ceux à qui il avait fait part de ses conjectures, que l'abeille approche de près mais pas exactement de la solution du problème du *maxima* et *minima*. Mais le fait est que l'abeille a raison, et que ce fut M. Koenig qui était tombé en erreur.

M'étant assuré que les angles sont ceux qu'a mesuré Maraldi, et que Koenig était tombé dans l'erreur par les tables de sinus

* Maraldi donne les angles comme 70° et 110° dans une partie de son mémoire, mais à ce qu'il semble approximativement ; car plus tard il donne 70° 32′, et 109° 28′ exactement : il paraît s'être trompé par avoir regardé le premier passage plus que le second.

ou des logarithmes, il m'a paru à propos de conduire l'investigation par chercher la longueur des côtés des rhombes, ou des autres lignes qui y ont rapport ; ce qui aurait le grand avantage d'éviter les erreurs en calculant les angles. Car non-seulement il est beaucoup plus facile de mesurer une petite ligne qu'un petit angle, mais il est évident que si la mesure des angles est exacte, la perpendiculaire d'un des angles des rhombes sur le côté opposé —c'est-à-dire, la largeur du rhombe—doit être égale au côté de l'hexagone ; et ainsi la mesure que seule il fallait faire, serait de constater l'égalité ou l'inégalité de ces deux lignes droites.

Nons pourrons résoudre le problème en cherchant ou la valeur de la perpendiculaire G G` = y, ou la valeur du côté du

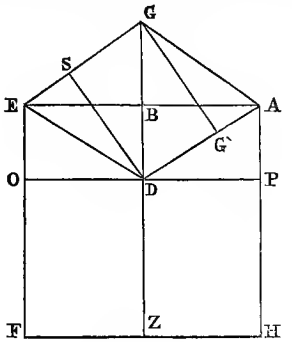


Fig. 3.

rhombe A D = x, qui donnera la surface du rhombe avec celle du trapèze 2 . E F Z D c'est-à-dire la surface entière G E F H A, le tiers de la surface de l'alvéole, un *minimum*. Prenons x pour le variable indépendant, et les rectangles O Z, P Z étant donnés et invariables, il faut chercher la valeur de x qui donne la somme du rhombe, des triangles A P D, E O D un *minimum*. Soit P D = S, le côté de l'hexagone ; par la propriété de cette figure, $AE = \sqrt{3} . S$, et $AB = \frac{\sqrt{3} . S}{2}$.

Partant $BD = \sqrt{4x^2 - 3S^2}$, et le triangle $ADB = \frac{\sqrt{3}S\sqrt{4x^2 - 3S^2}}{8}$,

et le rhombe A D E G $= \frac{\sqrt{3} \cdot S \sqrt{4x^2 - 3S^2}}{2}$. Mais A P $= \sqrt{x^2 - S^2}$, par conséquent le triangle A P D $= \frac{S\sqrt{x^2 - S^2}}{2}$, et la surface du rhombe avec celle des triangles A P D, E O D $= \frac{\sqrt{3} \cdot S}{2}$ $\sqrt{4x^2 - 3S^2} + S\sqrt{x^2 - S^2}$, dont le différentiel, $\frac{2\sqrt{3} \cdot S \cdot dx}{\sqrt{4x^2 - 3S^2}}$ $+ \frac{S\,dx}{\sqrt{x^2 - S^2}}$ doit = O pour trouver le *minimum*, et cela nous donne $x = \frac{3S}{2\sqrt{2}} =$ A D. Mais le rhombe A D E G aussi = G G` × A D; par conséquent G G` $= \frac{\text{rhombe}}{\text{A D}} = \sqrt{3} \cdot S \frac{\sqrt{4x^2 - 3S^2}}{2x} = \frac{3S^2}{3S} = S$.

Ainsi le *minimum* est lorsque la perpendiculaire G G`, ou la largeur des rhombes, est égale au côté de l'hexagone. Mais pour trouver les angles du rhombe, il faut considérer que les deux triangles E O D, S E D sont rectangulaires, et comme S D = O D, les angles D E O, D E S sont égaux; ainsi prenant D E pour rayon, nous avons E O pour le cosinus de O E D; et comme E D $= \frac{3S}{2\sqrt{2}}$, et O E $= \frac{S}{2\sqrt{2}}$, l'angle O E D est celui dont le cosinus est $\frac{1}{3}$ du rayon. Si celui-ci est 1,000,000, celui-là est 333,000; et dans la table de sinus naturels, le nombre le plus proche de 333,333 est 333,258, qui répond à l'angle 70° 32′. Ainsi c'est l'angle aigu du rhombe, et l'obtus est par conséquent 109° 28′. Effectivement l'égalité des angles O E D, que fait le rhombe avec le côté du prisme hexagone, est l'angle de 120°, que font les rhombes par leur inclinaison l'un à l'autre, détermine tout le reste, y compris les angles du rhombe D E S = D E O, et D S = D O, suffit à tout déterminer; et la comparaison des deux lignes D S,

D O est tout ce qu'il faut sans mesurer ni même calculer des angles, exceptez que l'on a celui de l'hexagone.*

Cherchons maintenant par un procédé semblable, les proportions des lignes et des angles, qui nous donnent un autre *minimum*, celui des angles dihédraux de l'alvéole. Ceci est très-important; car ces angles sont la partie de la structure qui demande le travail le plus difficile, et qui exige aussi la plus grande consommation de cire. Les parois sont plus épais aux angles parce que la solidité dépend plus des angles que des autres parties des parois. Or, la longuer de l'angle dihédrale de toute l'alvéole est $= 3\,A\,H + 3\,D\,Z + 6\,A\,D + 3\,A\,G$, on $3\,A\,H + 3\,DZ + 9\,A\,D$; on a donc à différencier $9x + 3\,(A\,H - \sqrt{x^2 - S^2})$. Ainsi $3\,dx - \frac{x\,dx}{\sqrt{x^2 - S^2}} = 0$, nous donne la valeur de x, côté du rhombe $= \frac{3\,S}{2\sqrt{2}}$, comme dans le problème pour la surface. Ainsi c'est la même proportion des côtés et des angles qui donne le *minimum* de ce travail si fin et si dispendieux de cire, c'est-à-dire la fabrication des angles, qui donne aussi le *minimum* des surfaces.

Les géomètres ont émis deux opinions opposées sur la différence entre le résultat de Koenig et la mesure de Maraldi. L'une est celle du justement célèbre Maclaurin,† qui avait résolu le problème par la géométrie élémentaire sans recourir au calcul; et trouvant les deux angles à quelques secondes

* Le rapport mérite attention du rhombe avec le triangle rectangulaire, bien connu des géomètres, dont les carrés des côtés sont dans la proportion de 1, 2, et 3.—Aussi le rhombe a un rapport remarquable avec la courbe Agnesième (La Versiera, ou Lutin), dont la Signora Agnesi a donné une construction très-élégante dans son ouvrage ('Instit. Analit.,' vol. I, p. 381): son équation est $Y = \frac{\sqrt{3}\,S}{4} \times \frac{4x^2 - 3S^2}{4x^2 + 3S^2}$, d'où l'on voit qu'elle doit être liée avec le rhombe. Effectivement si le circle générateur de la courbe est décrit sur l'un des diamètres du rhombe, avec un rayon du quart de ce diamètre, et la courbe a pour asymptote la tangente du circle, les ordonnés ont une proportion donnée aux cosinus de l'angle obtus, 109° 28′, ou aux sinus de l'angle aigu, 70° 32′.

† Trans. Phil. de Londres, 1742-3, p. 569.

près le même qu'a donné Maraldi, il a imputé l'erreur de Koenig aux tables de sinus dont il s'était servi. Mais ayant remarqué que Maraldi parle approximativement de 70° et 110° dans un passage de son mémoire, Maclaurin impute la diversité à un accident ou à la difficulté de mesurer ces petites quantités. L'autre opinion, ou plutôt doute, est du P. Boscowich, qui, penchant à croire que la mesure des angles sur une si petite échelle fût trop difficile pour être exacte, soupçonne Maraldi d'avoir commencé par calculer ou décliner leur grandeur d'une supposition qu'il eût faite de l'angle d'inclinaison des rhombes, 120°, et d'avoir fini par donner sa supposition comme le résultat de son mesurage. Cette opinion a été adoptée très-facilement par M. Castillon de Berlin et M. L'Huiller de Genève, dans leurs mémoires (Mém. de Berlin, 1781); et ils croient l'avoir confirmée par certaines mesures qu'ils donnent. Or, il n'est pas permis d'accuser Maraldi d'avoir donné comme le résultat de son mesurage, ce qui n'était qu'une conjecture, d'autant moins que n'ayant aucunement considéré la question d'un *minimum*, il ne pouvait pas avoir un préjugé pour une théorie favorite. Puis, à ce qui concerne les mesures de M. Castillon, elles ne valent rien, n'étant que deux à ce qui regarde la question disputée, et dont l'une plutôt soutient le calcul de Maraldi, ne faisant pas une différence plus de celle entre 4·144 et 4·168, qui n'est effectivement rien.

Mais ces deux géomètres ont soulevé d'autres difficultés sur la structure des alvéoles. Ils ont revoqué en doute le but principal de la construction, en niant que c'est pour économiser les matériaux et le travail, et prétendent que si c'était là l'objet, une épargne bien plus considérable aurait pu être gagnée en adoptant ce qu'ils appellent le *minimum minimorum*. Ils affirment que l'économie actuelle ne passe pas $\frac{1}{51}$ de la cire, et qu'avec une autre proportion de la profondeur à la largeur de l'alvéole, l'épargne aurait été beaucoup plus grande. Mais il est certain qu'ils se trompent sur tous les deux points.

1. Il n'est pas vrai de dire que l'épargne est d'un $\frac{1}{51}$, à moins que l'on impute dans la comparaison toute la cire des parois; cette comparaison tourne uniquement sur la différence entre la

base rhomboïdale et le prisme hexagone. La cire des parois ne peut pas entrer dans le calcul. S'il fut question entre deux espèces de toiture d'une maison de bois de sapin, de déterminer laquelle ferait le plus d'économie de bois, on ne mettrait jamais en ligne de compte les murs, pour savoir si l'économie serait d'un cinquième de toute la dépense. Cela ferait le calcul rouler sur la hauteur du bâtiment. De fait l'économie de cire et de travail est d'un $\frac{1}{8}$ au lieu du $\frac{1}{5\frac{1}{7}}$. Mais ce qui fait plus inexacte et même absurde l'importation des parois dans le calcul, c'est la différence marquée qui existe de l'épaisseur de différentes parties de l'alvéole. Le fond, la partie pyramidale est bien plus épaisse que les parois. J'ai très-souvent pesé des morceaux d'étendue égale des rhombes et triangles, et des parois adjacentes ; et j'ai trouvé que ceux-là avaient un poids de trois à deux en comparaison de ceux-ci. Il y a plus de variation entre les gâteaux en ce qui regard la différence d'épaisseur qu'il n'y en a eu ce qui regarde l'épaisseur des rhombes, mais si on est sur que la différence existe c'est assez pour détruire le calcul de M. L'Huiller. Si la proportion est de trois à deux, l'épargne monte au $\frac{1}{8}$ sur la partie la plus épaisse, et par conséquent à $\frac{1}{8\frac{1}{5}}$ au lieu de $\frac{1}{5\frac{1}{7}}$ sur la totalité, en emportant même, contre toute exactitude, les parois dans le calcul.

2. La question du *minimum minimorum*, dont M. L'Huiller cite un cas, dépend d'un problème, dont il n'a pas donné une solution générale. Il s'agit de trouver la proportion de la hauteur à la largeur de l'alvéole qui fasse la plus petite surface possible avec un contenu donné. Soit S = côté de l'hexagone ; M S = D S = perpendiculaire sur le côté opposé du rhombe d'un de ces angles ; y = A H, côté vertical du prisme ; Δ = contenu de l'alvéole. Partant, nous avons $y = \frac{2\Delta}{3\sqrt{3}.3^2}$;

rhombe $= \frac{3m\,S^2}{2\sqrt{3-m^2}}$; les triangles APD, DOE $= \frac{S^2\sqrt{4m^2-3}}{2\sqrt{3-m^2}}$;

la surface d'un tiers de l'alvéole $= S^2\left(\frac{3m-\sqrt{4m^2-3}}{2\sqrt{3-m^2}}\right) + 2Sy$

$= S^2\left(\frac{3m - \sqrt{4m^2-3}}{2\sqrt{3-m^2}}\right) + \frac{4\Delta}{3\sqrt{3}.S}$; et en différenciant et égalant à zéro on aura $S = \left(\frac{4\Delta\left(\sqrt{3-m^2}\right)^{\frac{1}{3}}}{3\sqrt{3}\left(3m - \sqrt{4m^2-3}\right)^{\frac{1}{3}}}\right)$, et par conséquent $S : y :: 2\sqrt{3-m^2} : 3m - \sqrt{4m^2-3}$. C'est le résultat générale pour toutes les constructions ; et dans le cas du *minimum*, quand D S = D O, ou $m = 1$, on a la proportion de $S : y :: \sqrt{2} : 1$. Donc la construction de l'alvéole sur ce principe donnerait la largeur et la profondeur comme $\sqrt{3} : 1$, ou comme $2\sqrt{2} : 1$ pour les deux largeurs de l'alvéole. Il y a une omission remarquable et fatale au résultat dans le calcul de M. L'Huiller, sur le cas particulier de $m = 1$. Mais avant d'en parler, il faut faire observer que la construction qui ferait l'alvéole presque trois fois plus large qu'elle n'est haute, ou profonde, serait entièrement incompatible avec chacun des objets auxquels l'alvéole doit servir. Par exemple, quoiqu'il serait possible d'y mettre les œufs, les vers ne pourraient pas être élevés, ni même exister. Encore la provision pour les insectes, et le miel lui-même, ne pourrait être amassé et gardé qu'en très-petite quantité. M. L'Huiller convient qu'il faut faire le sacrifice de l'épargne qu'il prétend résulterait de cette nouvelle construction, dont il ne nie pas que les inconvéniants plus que contrebalancent l'avantage qu'il suppose de l'épargne. Mais rien ne peut être plus contraire à tout principe que la conclusion qu'il déduit, que parce que, pour cette raison, l'économie de matériel est soumise aux objets principaux de toute la construction, cette économie n'entre pas de tout dans le plan et dans l'opération. Cette balance entre dans toutes les questions de *maximum* et *minimum* appliqués aux opérations naturelles. Mais même, en géométrie nous avons la même chose. S'il est question de trouver la proportion des deux côtés d'un rectangle, qui contenant une étendue donnée de surface, aurait ses côtés les plus courts, on sait que les côtés doivent être égaux. Mais personne ne dirait que la largeur de la figure n'entrât pas du tout dans notre considération, quoique

le but principal fut de déterminer la largeur, et qu'à ce but on avait sacrifié la largeur.

Mais jusqu'ici nous avons regardé le raisonnement de M. L'Huiller comme si sa solution du problème du *minimum minimorum* eut été exacte; au contraire, il n'a pas même posé la véritable question. Il a fait omission d'une partie de la surface, même très-importante, la plaque hexagone qui bouche ou ferme le tuyau; omission difficile à expliquer, excepté en croyant qu'il fût égaré par la question qu'avait soumis à Koenig, M. Réaumur. Mais dans cette question, la plaque hexagone ne pouvait pas entrer; étant construite, son expression aurait disparu de l'équation différentielle, dont s'est servi Koenig pour la solution. C'est tout autre chose dans la question du *minimum minimorum* qui fait la comparaison entre toutes les alvéoles. La plaque hexagone est une partie aussi essentielle que toutes les autres, au moins dans ces alvéoles qui gardent les provisions et le miel; probablement aussi dans celles qui entretiennent les vers, et qui sont l'habitation des chrysalides. Les vers surtout sont toujours couverts. Quand même il fut constaté que la nécessité de la couverture ou bouchon n'existe pas dans les alvéoles qui servent à l'entretien des chrysalides, comme elle est de toute nécessité dans les autres; il faudrait avoir deux espèces d'alvéoles; et ainsi la solution du problème ne serait bonne que pour cette espèce qui n'eut point de bouchon. Mais tout porte à croire qu'il n'y a qu'une espèce; car toute alvéole est employée indifféremment à toutes les opérations, et à tous les besoins de l'insecte.

Voyons donc quelle devait être la solution du problème. Elle est la même que celle qu'on a donné plus haut jusqu'à un certain point; et puis à l'expression différentielle il faut ajouter la valeur de la plaque hexagone $= \frac{3\sqrt{3}.S^2}{2}$. Le résultat est de nous donner la proportion de $S : y :: 2\sqrt{3-m^2} : 3m - \sqrt{4m^2-3} + \sqrt{3}\sqrt{3-m^2}$ pour toutes les proportions des lignes et angles; et dans le cas de l'abeille actuelle où $m = 1$, le *minimum minimorum* est, lorsque le côté de l'hexagone

est à la largeur du prisme dans la proportion de 2 à $\sqrt{2} + \sqrt{3}$ = on de 2·82 à 3·14 à peu près. Cette forme n'est pas aussi incompatible que celle qui résulte de l'autre solution, vu que la largeur de l'alvéole, quoique plus grande que sa profondeur, ne l'excède pas dans la même proportion. Pourtant la forme ne pourrait jamais convenir aux usages et aux nécessités de l'insecte ; et même il n'y aurait pas une économie de matériaux et de travail. Au contraire, on trouve en faisaut la comparaison que le montant de surface dans une alvéole ainsi construite est à celui d'une alvéole de la construction actuellement pratiquée, dans la proportiòn de S à y, de 1·387 à 5, et de 56, 52 à 49, 64. Ainsi il y a perte et non pas gain par la proportion du *mimimum minimorum.* Cette perte est de $\frac{1}{8}$ ou $\frac{1}{9}$ à peu près sur une seule alvéole ; mais sur le gâteau entier elle est assez grande. Mais ce n'est pas là la seule omission qu'ont font dans leurs conseils à l'abeille les Académiciens de Berlin. S'ils avaient fait attention à la différence en fait de travail aussi bien que de matériaux, de la fabrication des angles, ils auraient trouvé qu'il y a non-seulement comme on vient de faire voir un *minimum* en comparant les alvéoles de la même profondeur, mais qu'il y a aussi un *minimum* à ce qui regarde la surface. La même espèce d'investigation qui nous a conduit à l'un fait voir aussi l'autre. Si la comparaison est instituée entre les alvéoles du même contour on trouve la proportion du côté à la largeur du prisme qui donne la plus grande épargne d'angles, dans le cas de $m = 1$ (largeur du rhombe = côté de l'hexagone) est celle de $1 : \sqrt{2} + 1$. Il y a le même résultat si au lieu de la limite par la supposition du contenu donné, on prenne la surface du côté du prisme hexagone comme donné — limite qui n'est pas possible par la solution de l'autre problème de *minimum minimorum* pour la surface.* La longueur des angles dihédraux est 28, 92. Dans l'alvéole construite selon la proportion de 2 à $\sqrt{2} + \sqrt{3}$

* Il va sans dire qu'une limite est absolument nécessaire ; sans cela le plus court prisme serait celui qui ferait la plus grande épargne de surface et d'angles dihedraux.

(*minimum minimorum* de surface) la longueur est 47, 76 ; et dans l'alvéole actuelle (1387 : 5) 48 : 05. Ainsi il y a épargne d'angle dihédral dans ces deux cas en comparaison avec l'alvéole actuelle, surtout dans celle de $1 : \sqrt{2} + 1$. Mais les objections qu'on a pleinement indiquées font impossible de faire des alvéoles de cette forme. La construction ne serait pas si hors de proportion de la largeur à la hauteur que celle qu'ont proposé les académiciens de Berlin, de largeur à peu près trois fois plus grande que la hauteur ; et elle n'aurait pas causé une augmentation de surface. Mais pourtant elle aurait été en contradiction avec le but principal d'élever les insectes et de garder les provisions et le miel.

On ne peut pas douter de l'importance de tout ce qui démontre que les abeilles ont résolu le problème, et que leur architecture est plus exacte sous tous les rapports qu'aucune autre que l'on pourrait imaginer, si l'on réfléchit que c'est le chef-d'œuvre de toutes les opérations instinctives. Il est impossible de dire comme Virgile quand il a chanté les mœurs de l'abeille, "*In tenui labor*" sans ajouter "*at tenuis non gloria.*" Car il n'est pas permis de penser avec Descartes* que les animaux sont des machines. Au contraire, l'hypothèse, ou plutôt la doctrine Newtonienne† paraît plus fondée — que ce qu'on appelle instinct est l'action constante de Dieu ; et que ces speculations tendent à sa gloire, au moins à l'explication et à l'illustration raisonnée de ses œuvres et ses desseins. ‡

* 'Tract. de Méthode,' 36. Mais voir ses Lettres ; Epist., pars I, ch. 27.

† 'Optics,' lib. iii. ; Qu. 31. 'Principia,' lib. iii. 'Sch. Gen.'

‡ M. L'Huiller paraît être peu instruit sur l'histoire de ce fameux problème. Il dit (p. 280) que la solution du Père Boscowich est d'accord avec celle de Maclaurin.—'Phil. Trans.,' 1743. Mais c'est certain qu'il n'a jamais vu le mémoire de Maclaurin ; car il affirme que tous ceux qu'il nomme, y compris le Père B., aussi bien que Koenig, avaient été d'accord à regarder la question comme incapable de solution excepter par le calcul. Même s'arroge-t-il le mérite d'avoir le premier donné une solution par la géométrie ordinaire, quoiqu'il n'y a pas de doute qui Maclaurin l'avait donné près de quarante ans avant lui, et donné pour preuve de la force de la géométrie ancienne de laquelle il était un admirateur zélé.

VII.

EXPERIMENTS AND INVESTIGATIONS ON LIGHT AND COLOURS.

THE optical inquiries of which I am about to give an account, were conducted at this place in the months of November and December 1848, and continued in autumn 1849 at Brougham, where the sun proved of course much less favourable than in Provence: they were further prosecuted in October. I had thus an opportunity of carefully reconsidering the conclusions at which I had originally arrived; of subjecting them first to analytical investigation, and afterwards to repetition and variation of the experiments; and of conferring with my brethren of the Royal Society and of the National Institute. The climate of Provence is singularly adapted to such studies. I find, by my journal of 1848, that during forty-six days which I spent in those experiments, from 8 A.M. to 3 P.M., I scarcely ever was interrupted by a cloud, although it was November and December.* I have since had the great benefit of a most excellent set of instruments made by M. SOLEIL of Paris, whose great ingenuity and profound knowledge of optical subjects can only be exceeded by his admirable workmanship. I ought however to observe, that although his heliostate is of great convenience in some experiments, it yet is subject (as all heliostates must be) to the imperfection of losing light by reflexion, and consequently I

* Of seventy-eight days of winter in 1849, I had here only five of cloudy weather. Of sixty-one days of summer at Brougham, I had but three or four of clear weather; one of these fortunately happened whilst Sir D. Brewster was with me, and he saw the more important experiments.

have generally been obliged to encounter the inconvenience of the motion of the sun's image, especially when I had to work with small pencils of light. This inconvenience is materially lessened by using horizontal prisms and plates.

Although I have made mention of the apparatus of great delicacy which I employed, it must be observed that this is only required for experiments of a kind to depend upon nice measurements. All the principles which I have to state as the result of my experiments in this paper, can be made with the most simple apparatus, and without any difficulty or expense, as will presently appear.

It is perhaps unnecessary to make an apology for the form of definitions and propositions into which my statement is thrown. This is adopted for the purpose of making the narrative shorter and more distinct, and of subjecting my doctrines to a fuller scrutiny. I must further premise that I purposely avoid all arguments and suggestions upon the two rival theories—the Newtonian or Atomic, and the Undulatory. The conclusions at which I have arrived are wholly independent, as it appears to me, of that controversy. I cautiously avoid giving any opinion upon it; and instead of belonging to the sect of undulationists or anti-undulationists, I incline to agree with my learned and eminent colleague M. Biot, who considers himself as a "*Rieniste*," and neither "ondulationiste" nor "anti-ondulationiste."

Château Eleanor-Louise (Provence),*
1*st November*, 1849.

Definitions.

1. *Flexion* is the bending of the rays of light out of their course in passing near bodies. This has been sometimes termed *diffraction*, but *flexion* is the better word.

* In experiments at this place, in winter, I found one great advantage, namely, the more horizontal direction of the rays. In summer they are so nearly vertical, that a mirror must be used to obtain a long beam or pencil, which is often required in these experiments, and so the loss of light countervails the greater strength of the summer sun's light.

2. Flexion is of two kinds—*inflexion*, or the bending towards the body; *deflexion*, or the bending from the body.

3. *Flexibility*, *deflexibility*, *inflexibility*, express the disposition of the homogeneous or colour-making rays to be bent, deflected, inflected by bodies near which they pass.

Although there is always presumed to be a flexion and a separation of the most flexible rays from the least flexible (the red from the violet for example) when they pass by bodies, yet the compound rays are not so presumed to be decomposed when reflected by bodies. This is probably owing to the successive inflexions and deflexions before and after reflexion, correcting each other and making the whole beam continue parallel and undecomposed instead of becoming divergent and being decomposed.

Proposition I.

The flexion of any pencil or beam, whether of white or of homogeneous light, is in some constant proportion to the breadth of the coloured fringes formed by the rays after passing by the bending body. Those fringes are not three, but a very great number, continually decreasing as they recede from the bending body, in deflexion, where only one body is acting; and they are real images of the luminous body by whose light they are formed.

Exp. 1. If an edge be placed in a beam or in a pencil of white light, fringes are formed outside the shadow of the edge and parallel to it, by deflexion. They are seen distinctly to be coloured, the red being furthest from the shadow, the violet nearest, the green in the middle between the red and the violet. The best way to observe this is to receive the light on an instrument composed of two vertical and two horizontal plates, each moving by a screw so as to increase or lessen the distance between the opposite edges. a, a' are (fig. 1) the vertical, b, b' the horizontal edges, s, s are the screws; and these may be fitted

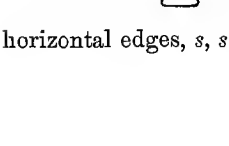
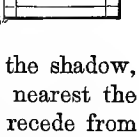


Fig. 1.

with micrometers, so as to measure very minute distances of the edges by graduated scales B B′, B′ C. For the purpose of the present proposition the aperture only needs be considered, of about a quarter of an inch square. The light passing through this aperture is received on a chart placed first one foot, and then several feet from the instrument. The fringes are increased in breadth by inclining the chart till it is horizontal, or nearly so, when the fringes parallel to *b*, *b′* are to be examined, and holding it inclined laterally when the fringes parallel to *a*, *a′* are to be examined. It is also convenient to let the white light beyond the fringes pass through; and for this purpose, *a″*, *b″* being the figure of the instrument (fig. 2), and the light received on the chart, a hole may be made in its centre *o p q*, through which the greater portion of the white light may be suffered to pass. The fringes are plainly seen to run parallel to the edges forming them; as *o p* parallel to *b″* and *p q* parallel to *a″*. The reddish is farthest from the shadow, the bluish nearest that shadow; also the fringe nearest the shadow is the broadest, the rest decrease as they recede from the shadow into the white light of the disc. Sometimes it is convenient to receive the fringes on a ground glass plate, and to place the eye behind it. They are thus rendered more perceptible.

Fig.2.



When the edges are placed in homogeneous light, they are all of the colour which passes by any edge; and two diversities are here to be noted carefully. *First*, the fringes made by the red light are broader than those made by any of the other rays, and the violet are the narrowest, the intermediate fringes being of intermediate breadths. *Second*, the fringes made by the red are farthest from the direct rays, the violet nearest those rays, the intermediate at intermediate distances. This is plainly shown in the following experiment.

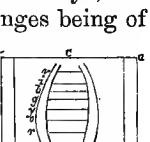


Fig.3.

Exp. 2. In fig. 3, C represents the image of the aperture when the rays of the prismatic spectrum are

made to pass through it. But instead of making the fringes by a single edge deflecting, and so casting them in the spectrum, I approach the opposite edges, so that both acting together on the light, the fringes are seen in the shadow and surrounding the spectrum. These fringes are no longer parallel to the shadows of the edges as they were in the white light, but incline towards the most refrangible and least flexible rays, and away from the least refrangible and most flexible. Thus the red part r of the fringes is nearest the shadow of the edge a'; the orange, o, next; then yellow, y; green, g; blue, b; indigo, i; and violet, v. Moreover, the fringe $r\,v$ is both inclined in this manner, so that its axis is inclined, and also its breadth increases gradually from v to r. This is a complete refutation of the notion entertained by some that Sir I. NEWTON's experiment of measuring the breadths in different coloured lights and finding the red broadest, the violet narrowest, explains the colours of the fringes made in white light as if these were only owing to the different breadths of the fringes formed by the different rays. The present experiment clearly proves, that not only the fringes are broadest in the least refrangible rays, but those rays are bent most out of their course, because both the axis of the fringes is inclined, and also their breadths are various.

Exp. 3. Though called by GRIMALDI, the discoverer, the three fringes, as well as by NEWTON and others who followed him, they are seen to be almost innumerable, if viewed through a prism to refract away the scattered light that obscures them. I stated this fact many years ago.*

Exp. 4. That the fringes are images may be at once perceived, not when formed in the light disc as in some of the foregoing experiments, but when formed in the shadow. Thus when the opposite edges are moved so near one another as to form fringes bordering the luminous body's image, they are formed like the disc they surround. When you view a

* Philosophical Transactions, 1797, part II.

candle through the interval of the opposite edges, you perceive that the fringes are images of its flame, with the wick, and that they move as the flame moves to and fro. When you observe the half-moon in like manner, you perceive that the side of the fringes answering to the rectilinear side of the moon, are rectilinear, and the other side circular; and when the full moon is thus viewed, the fringes on both sides are circular. The circular disc of the moon is, indeed, drawn or elongated as well as coloured. It is, that is to say, the fringe or image which is exactly a spectrum by flexion. Like the prismatic spectrum, it is oblong, not circular, and it is coloured; only that its colours are much less vivid than those of the prismatic spectrum.

Proposition II.

The rays of light, when inflected by bodies near which they pass, are thrown into a condition or state which disposes them to be on one of their sides more easily deflected than they were before the first flexion; and disposes them on the other side to be less easily deflected: and when deflected by bodies, they are thrown into a condition or state which disposes them on one side to be more easily inflected, and on the other side to be less easily inflected than they were before the first flexion.

Let R A (fig. 4) be a ray of light whose opposite sides are R A, R′ A′, and let A be a bending edge near which the ray passes, the side R′ A′ acquires by A's inflexion, a disposition to be more easily deflected by another body placed between A and the chart C, and the side R A acquires a disposition to be less easily deflected than before its first flexion; and in like manner R′ A′ acquires a disposition to be more easily inflected, and R A a disposition to be less easily inflected by a body placed between A and C.

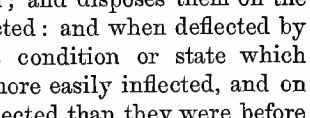
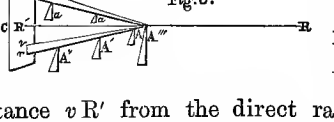
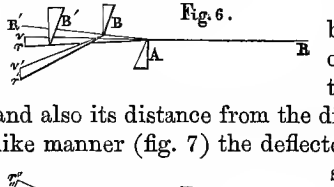


Fig. 4.

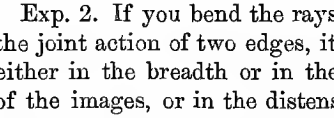
Exp. 1. Place A′ (fig. 5) in any position between A and *v r*,

the image made on C by A's influence, as at A′ or A″, or close to A at A‴. If it is placed on the same side of the ray with A, no difference whatever can be perceived to be made on the breadth of $r\,v$, or on its distance $v\,\mathrm{R}'$ from the direct ray R R′. In like manner the image by deflexion $r'\,v'$ is not affected at all, either in its breadth, or in its removal from R R′ by any object, a, a', placed on the same side with A of the deflected ray A v'.





But (fig. 6) place B anywhere between A and $v\,r$ on the side of the ray opposite to A, and the breadth of $r\,v$ is increased, and also its distance from the direct ray R R′, as $v'\,r'$; and in like manner (fig. 7) the deflected rays A v, A r are both more separated, making a broader image at $r''\,v''$, and are further removed from R R′ by B's inflexion.

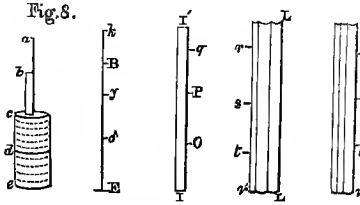


Exp. 2. If you bend the rays either by a single edge, or by the joint action of two edges, it makes not the least difference either in the breadth or in the distance from the direct rays of the images, or in the distension or elongation of the luminous body's disc, whether the bending body is a perfectly sharp edge (which in regard to the rays of light is a surface, though a narrow one), or is a plane (that is, a broader surface), or is a curve surface of a very small, or of a very large radius of curvature.

In fig. 8, $a\,e$ is an instrument composed of four pieces of different forms, but all in a perfectly straight line; $a\,b$ is an extremely sharp edge; $b\,c$ a flat surface; $c\,d$ a cylindrical or circular surface of a great radius of curvature; $d\,e$ one of a small radius of curvature. But all these pieces are so placed that E $\delta\,\gamma$ is a tangent to $e\,d$, $d\,c$, and is a continuation of $\gamma\,\beta$ K, that is, of $c\,b$, $b\,a$. So the light passing by the whole $a\,b\,c\,d\,e$, passes by one straight line E K, uniting or joining

the four surfaces. It is found that the image or fringe I I′, made by $a\,b\,c\,d\,e$ (or E δ γ B K), is of the same breadth and in the

Fig. 8.



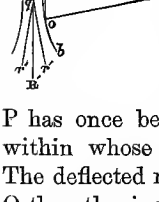
same position throughout its whole length. So if directly opposite to this edge another straight edge is placed, and acts together with $a\,b\,c\,d\,e$ on the light passing, the breadth of the fringe I is increased, and its distance is increased from the direct rays, but it has the exact same breadth from I to I′; its portion I′ q answering to $a\,b$, q P answering to $b\,c$, P O answering to $c\,d$, and O I answering to $d\,e$, are of the same breadth, provided care be taken that the second edge is exactly parallel to the edge E K. And this experiment may be made with the second edge behind $a\,b\,c\,d\,e$, as in Exp. 1 of this proposition; also it may be usefully varied by having the second edge composed of four surfaces like the first, only it becomes the more necessary to see that this compound edge is accurately made and kept quite parallel to the first, any deviation, however minute, greatly affecting the result. When care is thus used the fringes are as in $r\,v$, $v'\,r'$, quite the same in breadth and in position through their whole length; and not the least difference is to be discerned in them, whether made by a second edge, which is one sharp edge, or by a compound second edge, similar to $a\,b\,c\,d\,e$.

Hence I conclude that the beam passing by the compound edge, or compound edges, is exactly as much distended by the different flexibility of the rays, and is exactly as much bent from its direct course when the flexion is performed by a sharp edge, by a plane surface, by a very flat cylinder, or by a very convex cylinder; and therefore that all the action of the body on the rays is exercised by one line, or one particle,

and not first by one and then by others in succession; and this clearly proves that after a first flexion takes place, no other flexion is made by the body on the same side of the rays. This is easily shown.

For a plane surface is a series or succession of edges infinitely near each other; and a curve surface in like manner is a succession of infinitely small and near plane surfaces or edges. Let $a\,b$ (fig. 9) be the section of such a curve surface.

Fig. 9.



The particle P coming first near enough the ray R R′ to bend it, then the next particle O is only further distant from R R′, the unbent ray, than the particle P by the versed sine of the infinitely small arch O P. But O is not at all further distant than P from the ray bent by P into $q\,r$, and yet we see that O produces no effect whatever on the ray after P has once bent it. No more do any of the other particles within whose spheres of flexion the ray bent by P passes. The deflected ray $q\,r'$ no doubt is somewhat more distant from O than the incident ray was from P, but not so far as to be beyond O's sphere of deflexion; for O acts so as to make the other fringes at greater distances than the first. Consequently O could act on the first fringe made by P as much as P can in making the second, third, and other fringes; and if this be true of a curve surface, it is still more so of a plane surface; all whose particles are clearly equidistant from the ray's original path, and the particles after the first are in consequence of that first particle's flexion nearer the bent ray, at least in the case of inflexion. But it is to be observed, moreover, that in the experiment with two opposite edges, inflexion enters as well as deflection, and consequently this demonstration, founded on the exact equality of the fringes made by compound double edges, appears to be conclusive. For it must be observed that this experiment of the different edges and surfaces, plane and curve, having precisely the same action, is identical with the former experiment of two edges being placed one behind the other, and the second producing no effect if placed on

the same side of the ray with the first edge. These two edges are exactly like two successive particles of the same surface near to which the rays pass. Consequently the two experiments are not similar but identical; and thus the known fact of the edge and the back of a razor making the same fringes, proves the polarization of the rays on one side. Thus the proposition is proved as to polarization.

Exp. 3. The proposition is further demonstrated, as regards disposition, in the clearest manner by observing the effect of two bodies, as edges, whether placed directly opposite to each other while the rays pass between them so near as to be bent, or placed one behind the other but on opposite sides of the rays. Suppose the edges directly opposite one to the other, and suppose there is no disposition of the rays to be more easily bent by the one edge in consequence of the other edge's action. Then the breadth and distension and removal of the fringes caused by the two edges acting jointly, would be in proportion to the sum of the two separate actions. Suppose that one edge deflects and the other inflects, and suppose that inflection and deflexion are equal at equal distances, following the same law; then the force exerted by each edge being equal to d, that exerted by both must be equal to $2\,d$. But instead of this we find it equal to $5\,d$, or $6\,d$, which must be owing to the action of the two introducing a new power, or inducing a new disposition on the rays beyond what the action of one did.

If, however, we would take the forces more correctly (fig. 10), let A and B be the two edges, and let their spheres

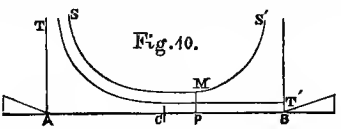


Fig. 10.

of flexion be equal, A C (= a) being A's sphere of inflexion and B's sphere of deflexion; B C (= a) being A's sphere of deflexion and B's sphere of inflexion; and let the flexion in each case be inversely as the mth power of the distance. Let

C P = x, P M = y, the force acting on a ray at the distance $a = x$ from A and $a - x$ from B. Then if B is removed and only A acts, $y = \frac{1}{(a+x)^m}$. If B also acts, $y' = \frac{1}{(a+x)^m} + \frac{1}{(a-x)^m}$.

Now the loci of y and y' are different curves, one similar to a conic hyperbola, the other similar to a cubic; but of some such form when $m = 1$, as S S′ and T T′. It is evident that the proportion of $y : y'$ can never be the same at any two points, and consequently that the breadths of the fringes made by the action of one can never bear the same proportion to the breadths of those made by the action of both, unless we introduce some other power as an element in the equation, some power whereby from both values, y and y', x may disappear, else any given proportion of $y : y'$ can only exist at some one value of x. Thus suppose (which the fact is) $y : y' :: 1 : 5$ or $1 : 6$, say $:: 1 : 6$, this proportion could only hold when

$$x = \frac{\left(5^{\frac{1}{m}} - 1\right) a}{5^{\frac{1}{m}} + 1} \text{ or } = \frac{\left(4^{\frac{1}{m}} - 1\right) a}{4^{\frac{1}{m}} + 1}, \text{ if } y : y' :: 1 : 5.$$

When $m = 2$, the force being inversely as the square of the distance, then $x = \frac{a}{3}$ and $x = \frac{(\sqrt{5} - 1)}{\sqrt{5} + 1} a$, are the values at which alone $y : y' :: 1 : 5$ and $1 : 6$ respectively.

But this is wholly inconsistent with all the experiments; for all of these give nearly the same proportion of $y : y'$ without regard to the distance, consequently the new element must be introduced to reconcile this fact. Thus we can easily suppose the conditions, *disposition* and *polarization* (I use the latter term merely because the effect of the first edge resembles polarization, and I use it without giving any opinion as to its identity), to satisfy the equation by introducing into the value of y some function of $a - x$. But that

the joint action of the two edges never can account for the difference produced on the fringes, is manifest from hence, that whatever value we give to m, we find the proportion of $y' : y$ when $x = 0$ only that of double, whereas 5 or 6 times is the fact. The same reasoning holds in the case of the spheres of flexion being of different extent; and there are other arguments arising from the analysis on this head, which it would be superfluous to go through, because what is delivered above enables any one to pursue the subject. The demonstration also holds if we suppose the deflective force to act as $\frac{1}{n}$ of the distance, while that of inflexion acts as $\frac{1}{m}$. But I have taken $m = n$ as simpler, and also as more probably the fact.

I have said that the rays become less easily inflected and deflected; but it is plain that on the polarized side they are not inflected or deflected at all. Their disposition on the opposite side is a matter of degree; their polarization is absolute and their flexion null.

Proposition III.

The rays disposed on one side by the first flexion are polarized on that side by the second flexion, and the rays polarized on the other side by the first flexion are depolarized and disposed on that side by the second flexion.

This proposition is proved by carefully applying the first experiment of Prop. II.; but great care is required in this experiment, because when three edges are used consecutively, the third edge often appears to act on rays previously acted on by both the other two, when it is only acting on those previously acted on by one or other of those two. Thus when edge A has inflected and edge B afterwards deflects the rays disposed by A, a third edge C may, when applied on the side opposite to B, seem to increase the flexion, and yet on removing A altogether we may find the same effect continue, which proves that the only action exercised had been by B and C, and that C had not acted on rays previously bent by

both A and B, which the experiment of course requires to prove the proposition. I was for a long while kept in great uncertainty by this circumstance, whether the third edge ever acted at all. That it never acted on the side of the ray on which the second edge acted, I plainly saw; but I frequently changed my opinion whether or not it acted on the opposite side, that is, on the same side with the first edge. Nor could I confidently determine this important point until I had the benefit of an instrument which I contrived for the purpose, and which, executed by M. Soleil, enabled me satisfactorily to perform the *experimentum crucis* as follows:—

In fig. X. A B is a beam, on a groove (of which the sides are graduated) three uprights are placed, the one, B, fixed, the

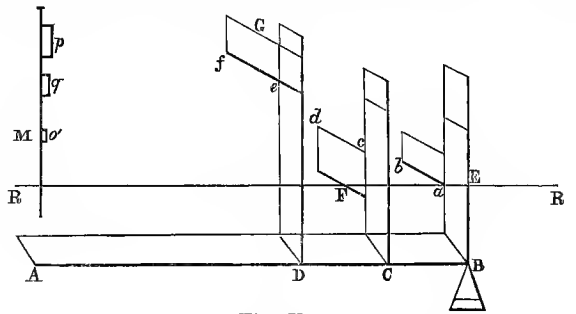


Fig. X.

other two, C and D, moving in the groove of A B. On each of the uprights is a broad sharp-edged plate, moving up and down the upright by a rack and pinion, so that both the plates F G could be approached as near as possible to each other, and so could F be approached to the plate E on the fixed upright B; while also each of the three plates could be brought as near the rays that passed as was required; and so could each be brought as near the opposite edge of the neighbouring plate. It is quite necessary that this instrument should be heavy in order to give it solidity: it is equally necessary that the rack and pinion movement should be just and also easy; for the object is to fix the plates at will, so

that their position in respect of the rays may be easily changed, and when once adjusted may be immovable until the observer desires to change their position.

The light was passed under the plate E and acted upon by *a b*, its lower edge. The second plate F was then raised on C so as to act on the side of the rays opposite to *a b*, by its upper edge *c d*. The fringes inflected by *a b* were thus deflected by *c d*, in virtue of the disposition given to the side next *c d*. Then the third plate G, on its stand D, was moved so that it could be brought to act by its lower edge *e f*, which was approached to the rays deflected by *c d*, and placed on their opposite side. The action was observed by examining the fringes on the chart M. Those which had been as *o*, made by the joint action of the two first edges E F, were seen to move upwards to *p* as the third edge G came near the rays; and *p* was both broader than *o*, and further removed from the direct rays R R′. In order to make quite sure that this change in the size and position of *o* had not been occasioned by the mere action of two plates, as E and G or F and G, it was quite necessary to remove first E, by drawing it up the stand B. If the fringe *p* then vanished, complete proof was afforded that E had acted as well as G. Then F was removed, and if *p* vanished, proof was afforded that F acted as well as E and G. A very convenient variation of the experiment was also tried and was found satisfactory. When the joint action of F and G gave a fringe, as at *q*, E being removed up the stand B, then E was gently moved down that stand, and as it approached the pencil, which was on its way to F and G, you plainly perceived the fringe enlarged and removed from *q* to *p*. These experiments were therefore quite crucial, and demonstrated that all the edges had concurred to form the fringe at *p*, the first and third inflecting, the second deflecting.

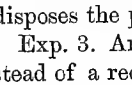
The same experiments were made on the fringes formed by the deflexion of the first edge and the inflexion of the second, and the deflexion of the third.

It is thus perfectly clear that the rays bent by the first

edge and disposed on their side opposite to that edge, are bent in the other direction by the second edge acting on that opposite side, and are afterwards again bent in the direction of the first bending by the action of the third edge upon the side which was opposite the second edge and nearest the first edge. But this side is the one polarized by the first edge, and therefore that side is depolarized by the action of the second edge. Hence it is proved that the rays polarized by one flexion are depolarized by a second; and as it is proved by repeated experiments that no body placed on the same side of the rays with any of the bending bodies, whether the first or the second or the third, exercises any action on those rays, it is thus manifest that any one flexion having disposed, a second polarizes the disposed side; and that any one flexion having polarized, a second flexion depolarizes and disposes the polarized side.

Exp. 3. Another test may be applied to this subject. Instead of a rectilinear edge, I made use of edges formed into a curve, as in fig. 12, where C is such an edge, and then the figure made is *g h*, corresponding to the curve *e b*. The first edge in the last experiment being formed like C, instead of a straight-lined edge, we can at once perceive that it has acted on the rays as well as the second and third edges, because these being straight-lined, never could give the comb-like shape *g h* to the fringes. This completely confirmed the other observations, and made the inference irresistible.

Fig. 12.



Proposition IV.

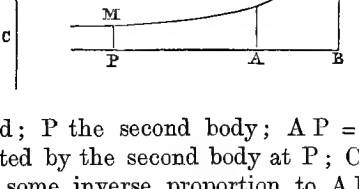
The disposition communicated by the flexion to the rays is alternative; and after inflexion they cannot be again inflected on either side; nor after deflexion can they be deflected. But they may be deflected after inflexion and inflected after deflexion, by another body acting upon the sides disposed, and not by its acting upon the sides polarized.

This is gathered from the experiments in proof of the second and third propositions.

PROPOSITION V.

The disposition impressed upon the rays, whether to be easily deflected or easily inflected by a second bending body, is strongest nearest the first bending body, and decreases as the distance between the two bodies increases.

Fig. 11. Let A B = a be the distance between the first bending body and a given point, more or less arbi-



trarily assumed; P the second body; A P = x; P M = y, the force exerted by the second body at P; C = the chart; P M = y is in some inverse proportion to A P, but not as $\frac{1}{\text{A P}^m}$ or $\frac{1}{x^m}$, because it is not infinite at A, but of an assignable value there; therefore $y = \frac{1}{(a+x)^m}$; and the curve which is the locus of P has an asymptote at B, when $x = -a$. The fringes being received on the chart at C, it might be supposed that the difference in their breadth, by which I measure the force, or y, is owing to P approaching the chart C, in proportion as it recedes from A, and thus making the divergence less in the same proportion; but the experiments are wholly at variance with this supposition.

Exp. 1. The following table is the result of one such experiment. The first column contains the distances horizontally of P from A, being the sines of the angle made by the rays with the vertical edges; the second column contains the real distance of the second from the first edge, the secant of that angle; the third column gives the breadths of the fringes at the distances given in the preceding columns; the

fourth gives the value of y, supposing M N were a conic hyperbola.

Sines.	Secants.	Real value of y.	Hyperbolic value.
20	35	$3\frac{1}{2}$	$3\frac{1}{2}$
65	85	$1\frac{2}{3}$	$1\frac{11}{17}$
85	$107\frac{1}{2}$	$1\frac{1}{2}$	$1\frac{1}{3}$
195	240	$0\frac{1}{2}$	$0\frac{7}{12}$

The unit here is $\frac{1}{20}$th of an inch.

It is plain that this agrees nearly with the conic hyperbola, but in no respect with a straight line; and upon calculating what effect the approach of P to C would have had, nothing could be more at variance with these numbers. But

Exp. 2. All doubt on this head is removed by making P the fixed point, and moving the first edge A nearer or further from it. In this experiment, the disturbing cause, arising from the varying distance from the chart, is entirely removed; and it is uniformly found that the decrease in the force varies notwithstanding with the increase of the distance. I have here only given the measures by way of illustration, and not in order to prove what the locus of y (or P) is, or, in other words, what the value of m is.

Exp. 3. When one plate with a rectilinear edge is placed in the rays, and a second such plate is placed at any distance between it and the chart, the fringes are of equal breadth throughout their length, and all equally removed from the direct rays, each point of the second edge being at the same distance from the corresponding point of the first. But let the second plate be placed at an angle with the first, and the fringes are very different. It is better to let the second be parallel to the chart, and to incline the first; for thus the different points of the fringes are at the same distance from the edge which bends the disposed rays. In fig. 13, B is the second plate, parallel to the chart C; A is the first plate;

all the points of B, from D to E, are equidistant from C; therefore nothing can be ascribed to the divergence of the bent rays. B bends the rays disposed by A at different distances D D′ and E E′ from the point of disposition. The fringe is now of various breadths from $d d'$ to e, the broadest part being that answering to the smallest distance of D, the point of flexion, from D′ the point of disposition; the narrowest part, e, answering to E E′, or the greatest distance of the point of flexion from the point of disposition. Moreover, the whole fringe is now inclined; it is in the form of a curve from $d d'$ to e, and the broad part $d d'$, formed by the flexion nearest the disposition, is furthest removed from the direct rays; the narrowest part, e, is nearest these direct rays. It is thus quite clear that the flexion by B is in some inverse proportion to the distance at which the rays are bent by B from the point where they were disposed by A. I repeatedly examined the curve $d e$, and found it certainly to be the conic hyperbola. Therefore $m = 1$, and the equation to the force of disposition is $y = \frac{1}{x}$.

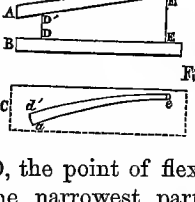


Fig. 13.

In order to ascertain the value of m, I was not satisfied with ordinary admeasurements, but had an instrument made of great accuracy and even delicacy. It consisted of two plates, A and B (Plate VI.), with sharp rectilinear edges, one, A, horizontal, the other, B, moving vertically on a pivot, and both nicely graduated. The angle at which the second plate was vertically inclined to the first, was likewise ascertained by a vertical graduated quadrant E. Moreover the edges moved also horizontally, and their angle with each other was measured by a horizontal graduated quadrant K. There was a fine micrometer F to ascertain the distances of the two edges from each other, and another to measure the breadth of the fringes on the chart. The observations made with this instrument gave me undoubted assurance that the

equation to the curve M N in fig. 11 is $y x = a$, a conic hyperbola, and that the disposing force is inversely as the distance at which the flexion of the rays bent and disposed takes place.

Scholium. — It is clear that the extraordinary property we have now been examining has no connexion with the different breadths of the pencils at different distances from the point of the first flexion, owing to the divergence caused by that flexion.

By the same kind of analysis, which we shall use in demonstrating the 6th Proposition, it may be shown,—*first*, that the divergence of the rays alone would give a different result, the fringes made by an inflexion following a deflexion and those made by a deflexion following an inflexion; *secondly*, that in no case would the equation to the disposing force be the conic hyperbola, even where that fringe decreased with the increase of the distance; *thirdly*, even where the effect of increasing the distance is such as the dispersion would lead to expect, the rate of decrease of the fringes is very much greater in fact than that calculation would lead to, five or six times as great in many cases; and *lastly*, that instead of the law of decrease being uniform, it would, if caused by the dispersion, vary at different distances from the two edges.* Nothing therefore can be more manifest than that the phenomena in question depend upon a peculiar property of the rays, which makes them change in their disposition with the length of the space through which they have travelled.

It should seem that light may be compared, when bent and thereby disposed, to a body in its nascent state, which, as we find by constant experience, has properties different from those which it has afterwards; and I have therefore contrived some experiments for the purpose of ascertaining whether or not light at the moment of its production (by

* I have given demonstrations of these propositions in a memoir presented to the National Institute, but I am reluctant to load the present paper with them.

artificial means) has properties other than those which it possesses after it has been some time produced. This will form the subject of a future inquiry. I would suggest, however, at present that the electric fluid ought to be examined with a view to find whether or not it has any property analogous to disposition, that is, whether it becomes more difficultly attracted at some distance from its evolution, as light is more difficultly bent at a distance from the point of its being disposed. On heat a like experiment may be made. The thermometer would no doubt stand at a different height at different distances from the source of the heat; but the question is if it will not reach its full height, whatever that may be, more quickly near its source than far from it. This experiment ought above all to be made on radiant heat, in which I confidently expect a property will be found similar to the disposition of light. It is also plain that we may expect strong analogies in magnetism and electro-magnetism. —I throw out these things because my time for such investigations may not be sufficiently extended to let me undertake them with success.

Proposition VI.

The figures made by the inflexion of the second body acting upon the rays deflected by the first, must, according to the calculus applied to the case, be broader than those made by the second body deflecting those rays inflected by the first.

In fig. 14, let A v' be the violet rays and A r' the red, inflected by A and deflected by B. Let A r be the red and A v the violet deflected by A and inflected by B. The action of B must inflect A r, A v into a broader fringe F, than the action of B deflects A v', A r' into the fringe f.

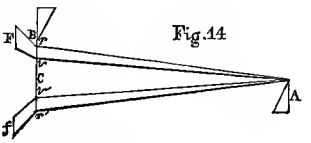


Fig. 14

Let B $r = a$ be the distance at which B acts on A r; $rv = d$ be the divergence of the red and violet; c be the distance of

the two bent pencils, and $v'\,r'$ the divergence of the inflected pencil, equal also to d, because we may take the different inflexibility to be as the different deflexibility. B acts on the red of $A\,r\,v$ as $\frac{r}{a^m}$; on the violet as $\frac{v}{(a+d)^m}$; and so on $A\,v'$ as $\frac{v}{(a+d+c)^m}$; on $A\,r'$ as $\frac{r}{(a+2\,d+c)^m}$. It is evident that the action in bending $A\,r$, $A\,v$, or the fringe made by that action, is to the fringe made by the action on $A\,r'$, $A\,v'$, as $\frac{r}{a^m} - \frac{v}{(a+d)^m} : \frac{r}{(a+2\,d+c)^m} - \frac{v}{(a+d+c)^m}$; and ultimately the two actions (or sets of fringes) are (supposing $a = 1$ and d also $= 1$, for simplifying the expression) as

$$2^m \times r\,(3+c)^m\,(2+c)^m - v\,(3+c)^m\,(2+c)^m \text{ to } 2^m\,r\,(2+c)^m - 2^m\,v\,(3+c)^m.$$

Now the former of these expressions must always be greater than the latter, because $(3+c)^m > 1$, and also $(3+c)^m - 1 > (2+c)^m - 1$; and this whatever be the value of m and of c, and whatever proportion we allow of r to v, the flexibilities. But it is also manifest that the excess of the first expression above the second will be greater if the flexibility of the red exceed that of the violet, or if r is greater than v, as $2\,v$. Hence we conclude; *first*, that in mixed or white light the fringes inflected by B after deflexion by A are greater than those deflected by B after inflexion by A; *secondly*, that they are also greater in homogeneous light; *thirdly*, that the excess of the inflected fringes over the deflected is greater in mixed than in homogeneous light.

The action of flexion after disposition is so much greater than that of simple flexion, that I have only taken into the calculation the compound flexion. But the most accurate analysis is that which makes the two fringes as

$$D + \frac{r}{a^m} - \frac{v}{(a+d)^m} \text{ to } D + \frac{r}{(a+2\,d+c)^m} - \frac{v}{(a+d+c)^m},$$

D being the breadth of the fringes on the chart by simple flexion in case the rays had passed on without disposition

and without a second flexion. If it be carefully kept in mind that D is much less than $\frac{r}{a^m}$, or even $\frac{r}{(a+2d+c)^m}$, and that d is still less than D, then it will always be certain that the first quantity is larger than the second.

Cor.—It is a corollary to this proposition that the difference of the two sets of fringes is increased by the disposition communicated by the rays in passing by the first body. For the excess of the value of r over that of v being increased, the difference between the two expressions is increased.

Proposition VII.

When one body only acts upon the rays, it must, by deflexion, form them into fringes or images decreasing as the distance from the bending body increases. But when the rays deflected and disposed by one body are afterwards inflected by a second body, the fringes will increase as they recede from the direct rays. Also when the fringes made by the inflexion of one body, and which increase with the distance from the direct rays, are deflected by a second body, the effect of the disposition and of the distances is such as to correct the effect of the first flexion, and the fringes by deflection of the second body are made to decrease as they recede from the direct rays.

In fig. 15, A P is the pencil inflected by A and forming the first and narrower fringe p; A r is the pencil inflected nearer to A and forming the broader fringe r. Such are the relative breadths, because they are inversely as some power of the distance at which A acts on them. But if B afterwards acts, it is shown by the same reasoning which was applied to the last proposition that r will be less than p; and so in like manner will r' be made less than o', though o' was greater than r' until B's action, and the effects of disposition with

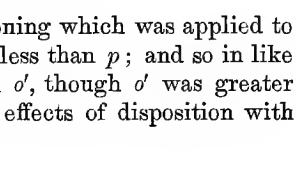


Fig. 15.

the greater proximity of the smaller fringe, altered the proportions.

Proposition VIII.

It is proved by experiment that the inflexion of the second body makes broader fringes or images than its deflexion after the inflexion of the first body; and also that the inflecto-deflexion fringes decrease, and the deflecto-inflexion fringes increase with the distance from the direct rays.

Exp. 1. It must be observed that when we examine the fringes (or images) made by the second edge deflecting the rays which the first had inflected, we can see the effects of the disposition communicated to the rays at a much greater distance of the second edge from the first, than we can perceive the effects of that disposition upon the inflexion by the second edge of the rays deflected by the first. Indeed we only lose the fringes thus made by deflexion, in consequence of their becoming so minute as to be imperceptible to our senses. But it is otherwise with the fringes or images made by the second edge inflecting the rays which the first had deflected. These can only be seen when the second edge is near the first, because the rays cannot pass on so as to form the images on the chart, if the second is distant from the first. The pencils diverge both by the deflexion and by the inflexion of the first edge. But we can always, when the inflected rays pass too far from the second edge, bring this so near them as to act on them, whereas we in so doing intercept the deflected rays. However, after this is explained, we find no difficulty in examining the effects of the inflexion by the second edge, only we must place it near the first and thus we have two sets of fringes, one extending into the shadow of the first edge at an inch distance between the two edges; but at an inch and three-fourths, nay, at two inches, or even more, this experiment can well be made.

Exp. 2. At these distances I examined repeatedly the comparative breadths of the two sets. In fig. 16, ab is the

white disc, on each side of which are fringes; those on the one side, $b\,c$, $c\,d$, are by the inflexion of the second edge; those on the opposite side, $a\,f$, $f\,e$, are by the deflexion of that second edge. I repeatedly measured these sets of fringes, and at various distances from the second edge; and I always found them much broader on the side of the second edge than on the opposite side. Thus $a\,b$ being the breadth of 5, $b\,c$ was 3, and $c\,d$ $4\frac{1}{2}$, while, on the opposite side, $a\,f$ was = 1 and $f\,e$ only $\frac{3}{4}$ or $\frac{1}{2}$. The fringes by inflexion of the second edge also uniformly increased as they receded from $a\,b$, the direct rays, whereas the opposite fringes as constantly decreased.



Fig. 16.

Exp. 3. If however the distance between the two edges be reduced, it is observed that the disparity between the two sets of fringes decreases, and they become gradually nearly equal; and when the edges are quite opposite each other there is no difference observable in the two sets. Each ray is disposed and polarized alike and affected alike by the two edges, and no difference can be perceived between the two sets.

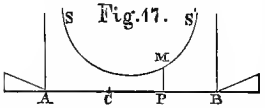
Exp. 4. The experiments also agree entirely with the calculus in respect of the relative values of r and v affecting the result. It appears that the fringes by the second edge's inflexion are broader than those by that edge's deflexion, whether we use white or homogeneous light. In the latter, however, the difference is not so considerable. This I have repeatedly tried and made others try, whose sight was better than my own. I may take the liberty of mentioning my friend Lord Douro, who has, I believe, hereditarily, great acuteness of vision.

Proposition IX.

The joint action of two bodies situated similarly with respect to the rays which pass between them so near as to be affected by both bodies, must, whatever be the law of their

action, provided it be inversely as some power of the distance, produce fringes or images which increase with the distance from the direct rays.

Let (fig. 17) A and B be the two bodies, and A C = C B = a be their spheres of flexion, so that A inflects and B deflects through A C, and A deflects and B inflects through C B. Let C P = x, P M = y. The force y, exerted by the joint action of A and B on any ray passing between them at P, is equal to $\frac{1}{(a+x)^m} + \frac{1}{(a-x)^n}$, supposing deflexion and inflexion to follow different laws. To find the minimum value of y, take its differential $dy = 0$; therefore we have



$$-m(a+x)^{-m-1}dx + n(a-x)^{-n-1}dx = 0, \text{ or } m(a-x)^{n+1} = n(a+x)^{m+1}.$$

If $m = n$ (as there is every reason for supposing), then $a - x = a + x$, or $x = 0$; and therefore, whatever be the value of m (that is whatever be the law of the force), the minimum value of y is at the point C where A's deflexion begins. The curve S S', which is the locus of M, comes nearest the axis at C, and recedes from that axis constantly between C and B. Hence it is plain that the fringes must increase (they being in proportion to the united action of A and B) from C to B; and in like manner must those made by B's deflexion and A's inflexion increase constantly from C to A; and this is true whatever be the law of the bending force, provided it is in some inverse ratio to the distance.

Proposition X.

It is proved by experiment that the fringes or images increase as the distance increases from the direct rays.

Exp. 1. Repeated observations and measurements satisfy us of this fact. We may either receive the images on a chart at various distances from the double edge instrument, approaching the edges until the fringes appear, or we may receive them on a plate of ground glass held between the sun and the

eye. We may thus measure them with a micrometer; but no such nicety is required, because their increase in breadth is manifest. The only doubt is with respect to their relative breadth when the edges are not very near and just when they begin to form fringes. Sometimes it should seem that these very narrow fringes decrease instead of increasing. However, it is not probable that this should be found true, at least when care is taken to place the two edges exactly opposite each other; because if it were true that at this greater distance of A from B (fig. 17) they decreased, then there must be a minimum value of P M between C and B, and between C and A; and consequently the law of flexion must vary in the different distances of A and B from the rays P, a supposition at variance it should seem with the law of continuity.

Exp. 2. The truth of this proposition is rendered more apparent by exposing the two edges to the rays forming the prismatic spectrum. The increase is thus rendered manifest. If the fringes are received on a ground glass plate, you can perceive twelve or thirteen on each side of the image by the direct rays. It is also worth while to make similar observations on artificial lights, and on the moon's light. The proposition receives additional support from these. But care must always be taken in such observations, which require the eye to be placed near the edges, that we are not misled by the effect of the small aperture in reversing the action of the edges. Thus when viewing the moon or a candle through the interval of two edges, one being in advance of the other, we have the coloured images (or fringes) cast on the wrong side. But if we are only making the experiment required to illustrate this proposition, the edges being to be kept directly opposite, no confusion can arise.

It is to be noted that the increase of breadth in the fringes is not very rapid in any of these experiments; nor are we led by the calculus to expect it. Thus suppose $m = 1$, we find $\left(\text{because } y = \frac{2a}{a^2 - x^2}\right)$ at the point C, when $x = 0$, the breadth

should be proportional to $\frac{2}{a}$. Take $x = \frac{a}{10}$, and the breadth is as $\frac{200}{99}$, or the breadth of the one fringe is to the other only as 200 to 198 or 100 : 99. We need not wonder therefore if there is only a gradual increase of breadth from C to B and from C to A. The increase is more rapid between $x = \frac{a}{2}$ and B than between C and $\frac{a}{2}$. Thus between the value of $x = \frac{a}{4}$ and $\frac{a}{2}$ the increase is as 4 : 5. But from $\frac{a}{2}$ to $\frac{3a}{4}$ the increase is as 7 : 12; and this too agrees exactly with the experiments; for as the edges are approached the increase of the fringes becomes more apparent.

Proposition XI.

The phenomena described in the foregoing propositions are wholly unconnected with interference, and incapable of being referred to it.

1. When the fringes in the shadow are formed by what is supposed to be interference, there are also formed other fringes outside the shadow and in the white light. If the rays passing on one side the bending body (as a pin or needle) are stopped, the internal fringes on the opposite side of the shadow are no longer seen. But no effect whatever is produced on the external fringes. These continue as long as the rays passing on the same side of the body on which they are formed, continue to pass. The external fringes have many other properties which wholly distinguish them from the internal or interference fringes.

2. Interference is said to be in proportion to the different lengths of the interfering rays, and not to operate unless those lengths are somewhat near an equality. In my experiments the second body may be placed a foot and a half away from the first, and the fringes by disposition are still formed,

though much narrower than when the bending bodies are more near to one another.

3. The breadth of the interference fringes is said to be in some inverse proportion to the difference in length of the interfering rays. It is commonly said to be inversely as that difference.

In fig. 20, A is the first and B the second edge. By interference the fringe at C should be broadest and at D narrowest, because A C − B C = A O is less than A D − B D = A P; and so as you recede from D, the fringes should become broader and broader, because the two rays become more nearly equal. But the very reverse is notoriously the case, the breadth of the fringes decreasing with their distance from the direct rays.

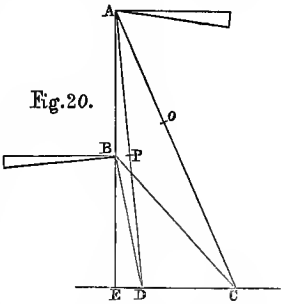


Fig. 20.

4. In the case of the fringes formed by the second body inflecting and the first deflecting, there can be no interference at all; for the whole action is on one and the same pencil or beam. A deflects and then B inflects the same ray; and when a third edge is placed on the opposite side to B, it only deflects the same ray, which is thus twice bent further from the direct rays, the last bending increasing that distance.

5. Let A be the first and B the second edge as before (fig. 20). Suppose B to be moveable, and find the equation to the disposing force at different distances of the two edges, we shall find this to be $y = \frac{1}{\sqrt{a^2+b^2} - \sqrt{(a-x)^2+b^2}}$, a being = A E, b = E D, and A B = x. But all the experiments show it to be $y = \frac{a}{x}$, a wholly different curve.

Again, let B be fixed, or the distance of the two edges be

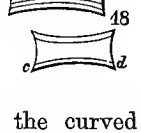
constant, we shall get the equation (a being = A E, b = B E, and E C = x) $y = \frac{1}{\sqrt{a^2 + x^2} - \sqrt{b^2 + x^2}}$, also a wholly different curve from the conic hyperbola, which all experiments give. Therefore the conclusion from the whole is that the phenomena have no reference to interference.

Having delivered the doctrines resulting from these experiments, I have some few particulars to add, both as illustrating and confirming the foregoing propositions, as removing one or two difficulties which have occurred to others until they were met by facts, and also as showing the tendency of the results at which we have arrived.

1. It may have been observed that in all those propositions I have taken for granted the inflexion of the rays by the body first acting upon them as well as their deflexion by that body, and have reasoned on that supposition. It is, however, not to be denied that we cannot easily perceive the fringes made by the single inflexion, as we can without any difficulty perceive those made by the single deflexion, and fully described in Proposition I. Sir I. NEWTON even assumes that no fringes are made within the shadow. I here purposely keep out of view the fringes made in the shadow of a hair or other small body, because the principle of interference there comes into play. However, I will now state the grounds of my assuming inflexion and separation of the rays by their different flexibility, when only a single body acts on them. In the *first* place, the first body does act in some way; for the second only acts after the first, and if the first be removed the fringes made in its shadow by the second at once vanish. *Secondly*, these fringes made by the second depend upon its proximity to the first. *Thirdly*, the following experiment seems decisive. Place, instead of a straight edge, one of the form in fig. 18, and then apply at some distance from it, the second edge, as in the former experiments. You find that the fringes assume the form, somewhat like a small-tooth comb,

of *a b*. If the second edge is furnished with a similar curve surface the form is more complete, as in *c d*. But the straight

Fig.18.



edge being used after the first flexion of the curved one, clearly shows that the first edge bends as well as the second, indeed more than the second, for the side of the figure answering to that curved edge is most curved. *Fourthly*, the whole experiments with two edges directly opposite each other negative the idea of there being no inflexion; indeed they seem to prove the inflexion equal to the deflexion. The phenomena under Proposition X. can in no way be reconciled to the supposition of the first edge not inflecting the rays.*

2. We must ever keep in view the difference between the fringes or images described by Sir I. NEWTON and measured by him, as made by the rays passing on each side of a hair, and the fringes or images which are made without the interference of rays passing on both sides. It is clear that the rays which form those fringes with their dark intervals do not proceed after passing the hair in straight lines. Sir I. NEWTON'S measures † prove this; for at half a foot from the hair he found the first fringe $\frac{1}{170}$th of an inch broad, and the second fringe $\frac{1}{290}$; and at nine feet distance the former were $\frac{1}{3\frac{1}{2}}$, the latter $\frac{1}{5\frac{1}{3}}$, instead of between $\frac{1}{9}$ and $\frac{1}{10}$, and the latter less than $\frac{1}{16}$, and so of all the other measures in the table, each being invariably about one-third what it ought to be if the rays moved in straight lines; and this also explains why the fringes do not run into one another, or encroach on the

* If you hold a body between the eye and a light, as that of a candle, and approach it to the rays, you see the flame drawn towards the body; and a beginning of images or fringes is perceived on that side.

† Optics, B. iii. obs. 3.

dark intervals in the case of the hair, as they must do if the rays moved in straight lines.

But the case of the fringes or images which we have been examining and reasoning upon is wholly different. I have measured the breadths of those formed by disposition and polarization, and found that they are broad in proportion to the distance from the bending edge of the chart on which they are received; and vary from the results given by similar triangles in so trifling a degree, that it can arise only from error in measurement. Thus in an average of five trials, at the relative distances of 41 and 73 inches, the disc was $6\frac{3}{5}$ at the shorter, and $10\frac{1}{5}$ at the longer distance; the fringe next it $3\frac{7}{10}$ at the shorter, and $5\frac{7}{10}$ at the longer distance, whereas the proportions by similar triangles would have been $9\frac{1}{6}$ and $5\frac{1}{6}$, so that the difference is small, and is by excess, and not, as in the hair experiment, by defect. Had the difference been as in Sir I. NEWTON's experiment, instead of $10\frac{1}{5}$ and $5\frac{7}{10}$, it would have $3\frac{1}{24}$ and $1\frac{7}{24}$. In another measurement at 101 and 158 inches respectively, the disc was $15\frac{1}{3}$, the fringe $8\frac{1}{3}$ instead of $14\frac{3}{5}$ and $9\frac{1}{3}$ respectively. But by Sir I. NEWTON's proportions these should have been $34\frac{3}{5}$ and $\frac{1}{24}$. It is plain that if the measures had been taken with the micrometer instruments, which had not been then furnished, there would have been no deviation. I have since tried the experiment, not as above, on the fringes formed by the double-edged instrument, but on those formed by one edge at a distance behind the other, and have found no reason to doubt that the rays follow a rectilinear course.

It may further be observed, that in the fringes or images by disposition and polarization, the dark intervals disappear at short distances from the point of flexion, and that the fringes run into one another, so that we find the red mixed with the blue and violet. This is one reason why I often experimented with the prismatic rays.

3. It follows from the property of light, which I have termed disposition, on one side the ray, and polarization on the opposite side, superinduced by flexion, that those two

sides only being affected, the other two at right angles to these are not at all affected by the flexion which has disposed and polarized the two former. Consequently, although an edge placed parallel to the disposing edge and opposite to it acts powerfully on the disposed light, yet an edge placed at right angles to the former edge or across the rays, does not affect them any more than it would rays which had not been subjected to the previous action of a first edge. Thus (fig. 19)

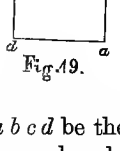


Fig. 19.

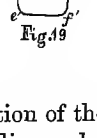


Fig. 19

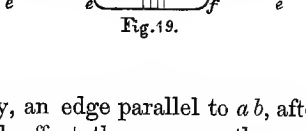


Fig. 19.

if *a b c d* be the section of the ray, an edge parallel to *a b*, after the ray has been disposed, will affect the ray greatly, provided it had been disposed by an edge also parallel to *a b*. The sides *a b* and *c d*, however, are alone affected; and therefore the second edge, if placed parallel to *a d* or *b c*, will not at all bend the ray more or make images (or fringes) more powerfully than it would do if no previous flexion and disposition had taken place. Let us see how this is in fact: *e f g h* is the distended disc after flexion, by passing through the aperture of the two-edged instrument (Plate XII.). It is slightly tinged with red at the two ends *f g* and *e h*, beyond which, and in the shadow of the edges, are the usual fringes or coloured images by flexion and disposition, *c*, *c*, the edges being parallel to *e h*, *f g*. Place another edge at some distance from the two, as 3 or 4 inches, and parallel to these two, but in the light, and you will see in the disc a succession of narrow fringes, parallel to the edges, and in front of the third edge's shadow. These fringes are on the white disc, and their colours are very bright, much more so than the colours of those fringes described in Proposition I., and which are fringes made by deflexion without any disposition. But whether this superior brightness is owing to the glare of the

disc's light being diminished by the flexion of the first two edges, or not, for the present I stop not to inquire. This is certain, that if the third edge be placed across the beam, and at right angles to the two first edges, you no longer have the small fringes. They are not formed in the direction $h\,g$, parallel to the edges as now placed. If the double edges are changed, and are placed in the direction $h'\,g'$, you again have the bright fringes; but then, if the third edge is now placed parallel to $h'\,e'$, you cease to have them. Care must, however, be taken in this experiment not to mistake for these bright fringes the ordinary deflexion fringes made by one flexion without disposition, as described in Proposition I. For these may be perceived, and even somewhat more distinctly in the disc than in the full light of the white pencil or beam.

Now are these bright fringes only the flexion fringes, that is fringes by simple flexion without disposition? To ascertain this I made these experiments.

Exp. 1. If they are the common fringes, and only enlarged by the greater divergence of the rays after flexion, and more bright by the dimness of the distended disc, then it will follow that the greater the distension, and the greater the divergence of the rays, the broader will be the bright fringes in question. I repeatedly have tried the thing by this test, and I uniformly find that increasing the divergence, by approaching the edges of the instrument, has no effect whatever in increasing the breadth of the fringes in question.

Exp. 2. If these fringes are not connected with disposition, it will follow that the distance of the edge which forms them from the double-edged instrument cannot affect them. But I have distinctly ascertained that their breadth does depend on that distance, and in order to remove all doubt as to the distance between the chart and the third edge which forms them, I allowed that edge to remain fixed, and varied its distance from the other two by bringing the double-edge instrument nearer the third edge. The breadths of the bright fringes varied most remarkably, being in some inverse power of that distance. Thus, to take one measurement as an

example of the rest, at 4 feet from the third edge the chart was fixed and the third edge kept constantly at that distance from it. Then the double-edge instrument was placed successively at $14\frac{1}{2}$, at 9 and at $4\frac{1}{2}$ eighths of an inch from the third edge. The breadths were respectively 2, $3\frac{3}{4}$ and $4\frac{1}{4}$ twentieths of an inch. In some experiments these measures approached more nearly the hyperbolic values of y, but I give the experiment now only for the important and indeed decisive evidence which it affords, that these fringes are caused by disposition, and are wholly different from those formed without previous flexion.

Exp. 3. If the greater breadth of these fringes is owing to dispersion, then they should be formed more in the rays of the prismatic spectrum than in white light, or even in light bent by flexion. Yet we find it more difficult to trace fringes across the prismatic spectrum than in white light, and more difficult across the spectrum when there is divergence, than when formed parallel to its sides when there is no divergence. There are fringes formed, but of the narrow kind, which are described in Prop. I.

Exp. 4. I have tried the effect on the fringes in question of the curvilinear edge described in the first article of these observations, and the effect of which is represented in fig. 18. It is certain that at a distance from the double-edge instrument the third edge seems only to form fringes rectilinear, or of its own form. But when placed very near, as half an inch from the instrument, plainly there is a curvilinear form given to the fringes in question; and this is most easily perceived, when, by moving the third edge towards the side of the pencil, you form the smaller fringes so as to be drawn across or along the greater ones made by the two first edges.

I think, without pursuing this subject further, it must be admitted that these fringes in light, which is bent and disposed, lend an important confirmation to the doctrine of disposition. It is clear that the rays are affected only on two of their four sides, or $a\,b$ and $c\,d$, if these are parallel to the bending body's edge, and not at all on the sides $c\,b$ and $d\,a$;

that, on the other hand, $c\,b$ and $d\,a$ are affected when the edges are placed parallel to these two sides of the rays; and thus the connection of the fringes in question, with the preceding action of which disposed and polarized, is clearly proved.

4. It is an obvious extension and variation of this experiment both to apply edges parallel to the first and disposing edges, and also to apply edges at right angles to their direction; and important results follow from this experiment. But until a more minute examination of the phenomena with accurate admeasurements can be had, I prefer not entering on this subject further than to say, that the extreme difficulty of obtaining fringes or images at once from the edges parallel to the first two, and from edges at right angles to these, indicates an action not always at right angles to the bending body, but whether conical or not I have not hitherto been able to ascertain. That the first body only disposes and polarizes in one direction is certain. But it seems difficult to explain the effect of the first two edges in preventing the fringes or images from being made by the second at right angles to those formed by the first two edges, if no lateral action exists. One can suppose the approaching of those two first edges to make the fringes narrower and narrower than those which the second two edges form when placed at right angles to the first. But this is by no means all that happens. There is hardly any set of fringes at all formed at right angles to the first set (parallel to the first two edges) when the first two are approached so near each other as greatly to distend the disc.

5. I reserve for future inquiry also the opinion held by Sir I. Newton, that the different homogeneous rays are acted upon by bodies at different distances, this action extending furthest over the least refrangible rays. He inferred this from the greater breadth of the fringes in those rays.

It is in my apprehension, though I once held a different opinion,* not impossible to account for the difference of the

* Philosophical Transactions, 1797.

breadth of the fringes by the different flexibility of the rays; and the reasoning in one of the foregoing propositions shows how this inquiry may be conducted. But one thing is certain, and probably Sir I. NEWTON had made the experiment and grounded his opinion upon the result. If you place a screen, with a narrow slit in the prismatic spectrum's rays, parallel to the rectilinear sides, and then place a second prism at right angles to the first and between the screen and the chart, you will see the image of the slit drawn on one side, the violet being furthest drawn, the red least drawn; but you will find no difference in the breadth of the image cast by the slit. Flexion, however, operates in a different manner, because it acts on rays, which, though of the same flexibility, are at different distances from the body.

6. The internal fringes in the shadow (said by interference) deserve to be examined much more minutely than they ever have been; and I have made many experiments on these, by which an action of the rays on one another is, I think, sufficiently proved. I shall here content myself with only stating such results as bear on the question of interference affecting my own other experiments. *First.* I observe that when one side of a needle or pin is grooved so as to be partly curvilinear, the other side remaining straight, we have internal fringes of the form in fig. 21. *Secondly.* It is not at all necessary the pin or other body forming them should be of very small diameter, although it is certain that the breadth of the fringes is inversely as the diameter. I have obtained them easily from a body one-quarter or one-third of an inch in diameter, but they must be received at a considerable distance from the body. *Thirdly*, and this is very material as to interference at all affecting my experiments, although certainly the internal fringes vanish when the rays are stopped coming from the opposite side of the object, the external fringes are not in the smallest degree affected, unless you stop the light coming on their own side; stopping the opposite rays has no effect whatever. Thus, stopping the

light on the side *a* (fig. 21), the fringes *ff* vanish, but not the external fringes *c*. This at once proves there is no interference in forming the external ones. *Lastly.* I may observe, that the law of disposition and polarization in some sort, though with modification, affects the internal fringes as well as the external.



Fig. 21.

It is a curious fact connected with polarization by inflexion, and which indeed is only to be accounted for by that affection of light, that nothing else prevents the rays from circulating round bodies exposed to them, at least bodies of moderate diameter. If the successive particles of the surface inflected, one particle acting after the other, the rays must necessarily come round to the very point of the first flexion. We should thus see a candle placed at A (fig. 22) when the eye was placed at B, because the rays would be inflected all round; and even in parts of the earth where the sea is smooth, nothing but the small curvature of the surface could prevent us from seeing the sun many hours after night had begun by placing the eye close to the ground. This, however, in bodies of a small diameter, must inevitably happen. The polarization of the rays alone prevents it, by making it impossible they should be more than once inflected on their side which was next the bending body, therefore they go on straight on to C. But for polarization they must move round the body.

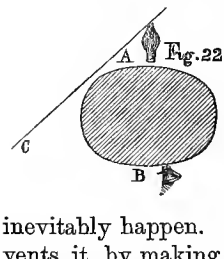


Fig. 22.

7. It must not be lightly supposed, that because such inquiries as we have been engaged in are on phenomena of a minute description and relate to very small distances, therefore they are unimportant. Their results lead to the constitution of light, and its motion, and its action, and the relations between light and all bodies. I purposely abstain from pursuing the principles which I have ventured to explain into their consequences, and reserve for another

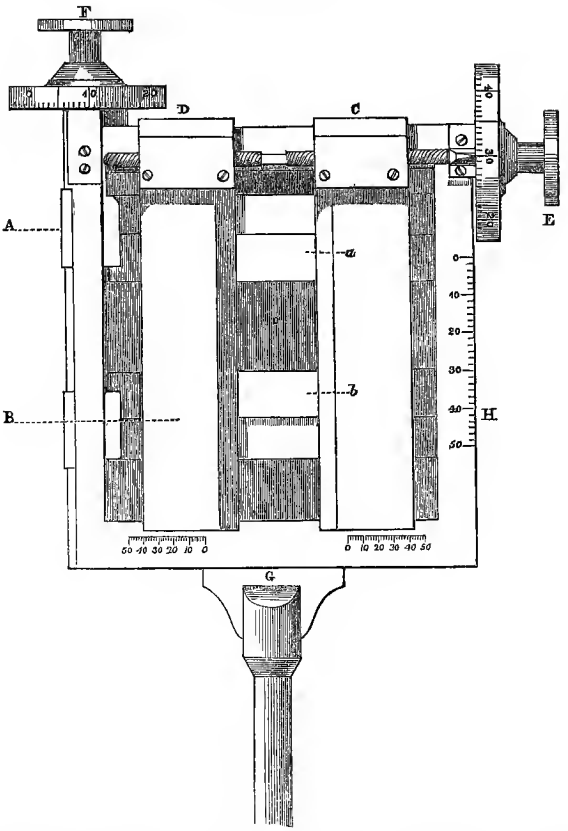
occasion some more general inquiries founded upon what goes before. This course is dictated by the manifest expediency of first expounding the fundamental principles, and I therefore begin by respectfully submitting these to the consideration of the learned in such matters.

In the meantime, however, I will mention one inference to be drawn from the foregoing propositions of some interest.

As it is clear that the disposition varies with the distance, and is inversely as that distance, and as this forms an inherent and essential property of the light itself, what is the result? Plainly this, that the motion of light is quite uniform after flexion, and apparently before also. The flexion produces acceleration but only for an instant. If ss is the space through which the ray moves after entering the sphere of flexion, and v the velocity before it enters that sphere; it moves after entering with a velocity $= \sqrt{v^2 + \mathrm{Z}\,dz}$, Z being the law of the bending force. Then this is greater than v; consequently there is an acceleration, though not very great; but because $y = \frac{a}{x}$, if s is the space, t the time, the force of acceleration is $\frac{s}{t\,ds} \times \frac{t\,ds - s\,dt}{t^2}$; but $y = \frac{a}{x}$ shows that s is as t, else $y = \frac{a}{x}$ would be impossible; therefore the accelerating force $\frac{s}{ds} \times \frac{t\,ds - s\,dt}{t^3} = 0$, and so it is shown there is no acceleration after the ray leaves the sphere of flexion.

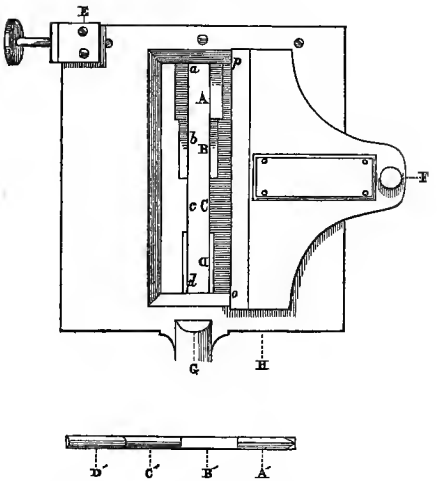
Description of the Instruments.

PLATE XII.



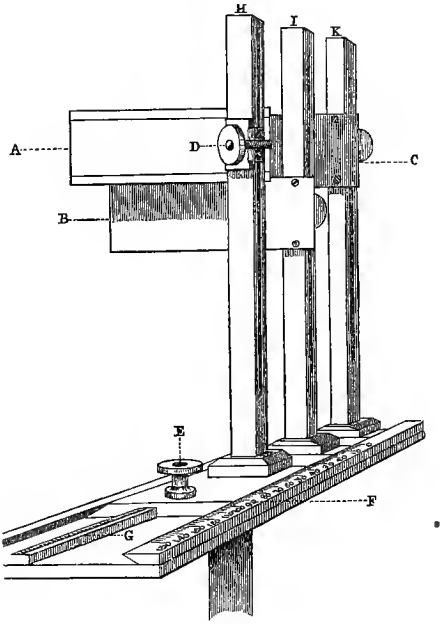
Is the instrument with two plates or edges. A, B, horizontal, D, C, vertical; the former moved by the screw E, which has also a micrometer for the distances on the scale G; the latter, in like manner, moved by F, connected with micrometer and scale H.

PLATE XIII.



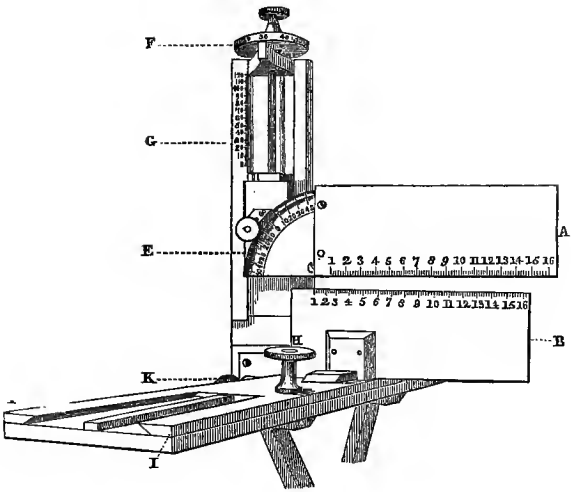
Is the instrument with four surfaces. A D, *a d* are two parallel plates, moving horizontally by a rack and pinion E. Each plate has an edge composed of four surfaces; A, *a*, a sharp edge or very narrow surface; B, *b*, a flat surface; C, *c*, a cylindrical surface of large radius of curvature, and so flat; D, *d*, one of small radius, and so very convex: this is represented on the figure by A′ B′ C′ D′ beside the other. Care is to be taken that A B C D and *a b c d* be a perfectly straight line, made up of the sharp edge, the plane surface and the tangents to the two cylinders. H is a plate with a sharp and straight edge, *o p*, which can be brought by its handle F to come opposite to the compound edge *a b c d*, when it is desired to try the flexion by the latter, without another flexion by an opposite compound edge, but only with a flexion by a rectilinear simple edge.

PLATE XIV.



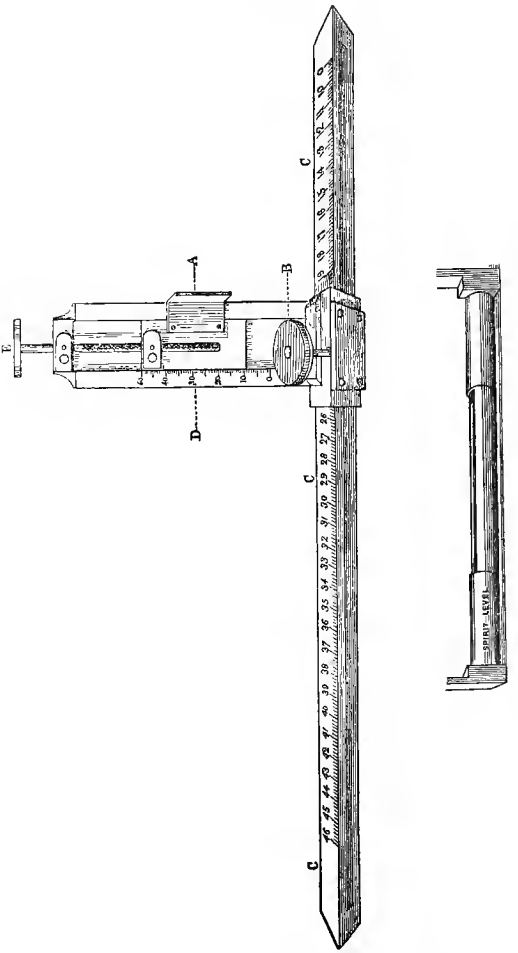
Is the instrument by which is tried the *experimentum crucis* on the action of the third edge, and also the experiments on the distances of the edges as affecting the disposing force. G is the groove in which the uprights H, I, K move. There is a scale graduated, F, by which the relative distances can always be determined of the plates A, C and B. A moves up and down upon H, B upon I, and C upon K; each plate is moved up and down by rack and pinion D. The uprights also move along the groove G by rack and pinion E.

PLATE XV.



Is the instrument for ascertaining more nicely the effects of distance on disposition. A is a plate with graduated edge; it moves vertically on a pivot, and its angle with the horizontal line is measured by the quadrant E. A also moves horizontally, and its horizontal angle is measured by the quadrant K. B is another plate with graduated edge, moving in a groove D, by rack and pinion H, and along a graduated beam I. F is a fine micrometer, by which the distance of A above B, when A is horizontal, can always be measured to the greatest nicety by the circle F and the scale G.

PLATE XVI.



Is an instrument also for measuring the effect of the distance of the edges upon the disposing forces. C C C is a graduated

beam, adjusted by the spirit-level, and on it moves the upright on which a plate A moves by micrometer screw E, so that the distance of A from the rays that pass along C C C after flexion by a plate fixed at one end of the beam, can be ascertained by the scale D. I have experimented with this, but I did not find it so easy to work by as the other apparatus. C C C is brought to an exact level by screws not noted in the drawing.*

* This Tract is from the Phil. Trans. for 1850, Part II.—See Note IV.

VIII.

RECHERCHES EXPÉRIMENTALES ET ANALYTIQUES SUR LA LUMIÈRE.

Newton, dans le troisième livre de l'*Optique*, donne ses expériences sur l'inflexion de la lumière ; et, examinant les bandes qu'avait décrites Grimaldi comme entourant les ombres des corps, Newton trouve qu'elles diffèrent de largeur dans les couleurs de spectre prismatique, qu'elles sont plus larges formées par les rayons les moins réfrangibles, plus minces formées par les rayons les plus réfrangibles, et d'une largeur moyenne dans les rayons intermédiaires. L'expérience par laquelle il voudrait établir cette proposition est le mesurage de la ligne entre les centres des bandes des côtés opposés de l'ombre. Il trouva qu'à la distance de six pouces cette ligne était de $\frac{1}{37}$ à $\frac{1}{38}$ de pouce pour les bandes rouges ; de $\frac{1}{46}$ de pouce pour les bandes violettes. Or, si les bandes sont à une distance égale de l'ombre dans toutes les couleurs, l'expérience est concluante. Si leurs distances sont différentes, l'expérience ne prouve rien ; des bandes moins larges mais plus distantes pourront donner la ligne entre leurs centres, plus grande que la ligne entre les centres des bandes plus larges, mais plus rapprochées de l'ombre. Donc évidemment Newton supposa que la distance des bandes était la même dans tous les rayons du spectre ; c'est-à-dire il regarda l'action des corps fléchissants comme formant des bandes de largeur diverse avec les diverses rayons, mais non pas comme fléchissant les rayons différemment de leur cours. En un mot, selon lui, l'angle de flexion est le même, quel que soit le rayon fléchi, qu'il soit rouge ou qu'il soit violet. La conclusion qu'il déduit de l'expérience est, non pas que le corps fléchit différemment les rayons, mais que son action ou son influence s'étend à des distances diffé-

rentes sur les différents rayons, plus loin sur les rayons les moins réfrangibles. Cependant, il n'y a pas de doute que les rayons ne soient fléchis différemment ; et comme cette position est assez importante en soi et dans ses suites, l'on me permettra d'en donner les preuves un peu en détail.

1° Que deux bords ou biseaux exactement parallèles soient placés dans les rayons du spectre prismatique, parallèlement à son axe et perpendiculairement à l'axe du prisme, et que les bandes soient observées lorsque les bords sont rapprochés l'un de l'autre : on voit clairement qu'elles ne sont pas parallèles entre elles, ni à l'axe ni aux côtés du spectre. Au contraire, elles inclinent vers le violet, et sont le plus éloignées, du spectre et le plus séparées entre elles dans les rayons rouges. Aussi leurs largeurs sont différentes, les rouges les plus larges, les violettes les moins larges, les autres de largeur moyenne. VR est le spectre, R étant la partie rouge, V la partie violette, $r\,v$, $r\,v$ sont les bandes d'un côté, inclinées du rouge r au violet v (fig. 1).

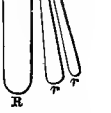


Fig 1

2° Cette expérience exige le parallélisme exact des bords, parce qu'un très-petit écart du parallélisme, en faisant que sur un point les bords se rapprochassent plus que sur d'autres points, ne manquerait pas d'augmenter la largeur et l'éloignement des bandes répondant à ce point-là. En effet, les bandes prendraient la forme hyperbolique si les bords s'inclinaient même très-peu l'un vers l'autre, et ainsi l'expérience deviendrait peu concluante ; c'est pourquoi il y a d'autres expériences (et qui ne sont pas exposées à la même objection) qu'il faut ajouter, après avoir fait observer que l'on peut vérifier l'expérience avec les bords, en renversant le prisme ou les bords eux-mêmes, de manière à faire passer les rayons violets par l'endroit où les bords sont soupçonnés n'être pas exactement parallèles, et par où les rayons rouges avaient passé avant.

3° La preuve de notre proposition est fournie par l'examen des bandes formées par un bord ou par un autre corps seul. Ces bandes, il est vrai, sont beaucoup plus petites et moins distantes de l'axe du spectre, ou des bords de l'ombre, que celles que forme l'action combinée de deux tranchants ; mais

elles varient tant en distance qu'en largeur dans les différentes couleurs, inclinant du rouge au violet. Pour pouvoir les observer distinctement, il est bon de les recevoir sur un tableau parallèle au corps fléchissant, mais incliné latéralement de manière à les grossir en largeur. Il doit être placé de 18 à 20 cent. du corps fléchissant, et celui-ci de 5 à 6 pieds du prisme. Aussi le pinceau réfracté doit être admis par une petite ouverture, pour éviter trop de lueur. Si les largeurs et les distances respectives sont mesurées, on les trouvera de la moitié plus grandes dans les bandes formées par les rayons les moins réfrangibles, que dans celles que forment les rayons les plus réfrangibles; et on trouvera leurs cours à travers les divers rayons du spectre rectiligne, ou à très-peu près, à ce qu'il me semble. Si leur cours est hyperbolique, c'est de cette courbe assez loin de l'origine ou de l'asymptote.

4° Si une aiguille ou autre corps mince est placé dans les rayons du spectre, on voit assez distinctement les bandes externes varier en largeur et en distance de l'ombre, les rouges étant les plus larges et les plus éloignées; les violettes, les plus minces et les plus proches. Les bandes internes ou de l'ombre semblent varier aussi, mais il y a une très-grande difficulté à les estimer. La ligne grise et obscure à l'axe de l'ombre est plus facile à examiner. Elle paraît être plus large là où elle répond aux parties rouges du spectre. A la partie des bandes internes répondant à la point de l'aiguille, et où ces bandes sont divergentes et se joignent aux bandes externes, la largeur et la séparation entre elles sont évidemment plus considérables dans les rouges, tant dans les bandes internes que dans les externes.

5° Lorsque dans ces expériences l'on examine les bandes bien près du corps fléchissant, il faut les recevoir sur un verre dépoli d'un côté, en plaçant l'œil derrière ce côté du verre.

6° Il y a une forme d'expérience que j'ai trouvée assez commode, tant en ce qu'elle peut toujours se faire, que parce qu'elle ne dépend aucunement du parallélisme des bords. Placez un prisme d'angle réfractant, de 60 dég. au moins, horizontalement à 5 à 6 pieds d'une bougie ou d'une lampe brûlant d'une petite flamme, et regardez le spectre colorié par deux

bords placés entre le prisme et l'œil. Les bandes ou images colorées de la flamme paraissent décroissantes en largeur et en distance de la flamme, étant plus larges et plus éloignées dans la partie rouge, plus minces et plus proches dans la partie violette. Lorsque les bords s'approchent de manière à faire distendre l'image de la flamme, elle paraît comme à la fig. 2, qui donne aussi les bandes d'un côté. Lorsque les bords sont encore plus rapprochés, le disque ou image centrale de la flamme est encore plus dilaté, et paraît divisé en deux avec un intervalle obscur ou noir *o* entre les deux, comme dans la troisième figure, qui ne donne pas les bandes, parce qu'elles ont presque disparu. Si l'on peut soupçonner que les bords ne sont pas parallèles, le prisme pourra être renversé, ou les bords pourront l'être : les bandes restent comme avant, excepté que si le prisme est renversé, le rouge doit être en bas et le violet en haut. Mais cette expérience ne peut être affectée par le défaut d'un parallélisme très-exact, vu que les rayons passent entre une fort petite portion des bords, pas plus qu'un $\frac{1}{20}$ de centimètre. Supposons que les bords ont une inclinaison même sensible, comme d'un angle de 30′, et que la flamme est regardée à travers des bords à la distance d'un angle de 10 cent.

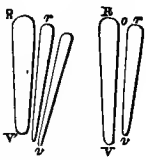


Fig.2. Fig.3

Soit P P′ (fig. 4), la partie des bords A P, A M, par laquelle les rayons du spectre passent à l'œil. Si P P′ n'est que de

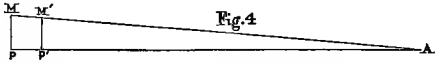


Fig.4

$\frac{1}{16}$ cent., M′ P′ n'est que $\frac{1}{1600}$ moins grand que M P, où les bords ne se rapprochent pas plus sur un point que sur un autre. Mais supposons que P P′ est plus considérable, disons de $\frac{1}{10}$ cent. au lieu de $\frac{1}{16}$, la différence entre M′ P′ et M P ne serait même alors plus que $\frac{1}{1000}$ cent. Or une différence même plus grande que celle-ci ne produit aucun effet sensible sur les franges, comme je l'ai bien des fois constaté dans des expériences avec le micromètre. Donc la preuve que l'on vient de donner est sous tous les rapports concluante, et aucune

erreur ne peut s'y introduire par le défaut de parallélisme des bords. Il est bien pourtant de regarder la flamme par une partie des bords pas trop près de l'angle, s'ils ont une inclinaison entre eux, parce que, bien que M P—M′ P′ est toujours, —quel que soit A P,—la même, pourvu que P P′ soit quantité constante, cependant la proportion de M P à M′ P′ varie avec la valeur de A P; et quelques expériences m'ont fait soupçonner que cette proportion variante, quand la différence reste constante, pourra influer sur les phénomènes.*

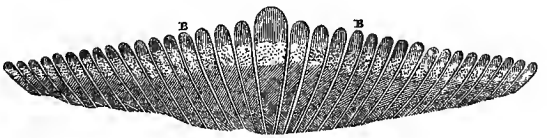
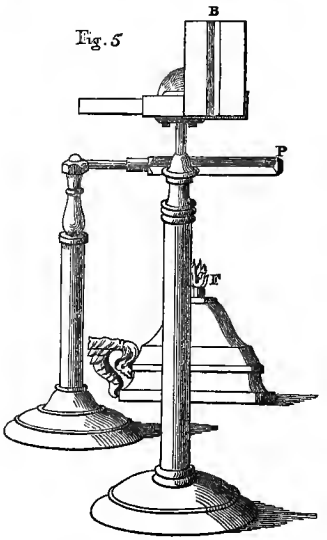


Fig. 5



* La cinquième figure donne l'expérience avec la flamme. F = la flamme; B = les bords; P = le prisme; B B = les bandes. Mais l'artiste qui les a dessinées paraît les avoir représentées un peu trop larges sur la partie supérieure.

7° Si nous fixons conjointement deux lames de verre coloré, l'une rouge et l'autre bleue, examinant ainsi les bandes formées de la lumière qui les traverse par deux bords placés derrière, et que nous regardions aussi le disque distendu par l'action des bords, nous trouvons la partie rouge de ce disque plus distendue que la partie bleue, et les bandes rouges plus larges et plus séparées les unes des autres.

8° L'augmentation de la distance en même temps que de la largeur dans les bandes rouges formées par un bord seul ou par les deux, paraît évidente de ce que si elles n'étaient qu'également éloignées de l'ombre du corps ou de l'axe de spectre, elles seraient parallèles à l'ombre ou à l'axe; et par conséquent il y aurait entre chaque bande et les bandes avoisinantes un intervalle croissant du rouge vers le violet, comme dans la sixième figure, où R V est le spectre, et *r v* sont les bandes. Or il n'y a rien de la sorte à voir, examinez les phénomènes comme vous voudrez. Les intervalles obscurs, on les voit toujours diminuer de largeur du rouge vers le violet ; et au violet ces intervalles sont si minces, qu'à peine peut-on les tracer.



Fig.6

9° La même diversité des rayons homogènes, je l'ai trouvée dans tous les autres cas des bandes, soit de celles qui sont formées par la flexion seule, soit de celles formées dans les expériences avec des spéculums, ou des surfaces striées par la flexion combinée avec la réflexion. Lorsque ces bandes sont formées par la lumière blanche, elles ont toutes les couleurs, et elles sont parallèles entre elles et à l'axe du pinceau ou de la flamme ; mais, formées par les rayons du spectre, elles sont toujours plus larges dans les rayons les moins réfrangibles, et ont une inclinaison sensible du rouge vers le violet. Ainsi les bandes d'une surface striée exposée à la lumière blanche étant, comme dans la septième figure, le rouge plus loin, le bleu plus près de la flamme,—ces mêmes bandes regardées par la réflexion des rayons du prisme sont, comme dans la huitième figure, plus larges et plus éloignées

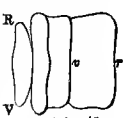


Fig.7.

de l'image R V de la flamme, dans leur portion rouge r, et s'approchent entre elles et de la flamme vers la portion violette v. De même les bandes formées par un miroir plane assez mince, et qui sont égales entre elles et parallèles aux bords du miroir, si elles sont formées dans la lumière blanche ou dans la lumière homogène (mais de même espèce en plaçant le miroir à travers le spectre), deviennent entièrement différentes si le miroir est placé perpendiculairement au prisme et parallèle au spectre; car alors elles sont plus larges dans les rouges, et plus distantes des bords du miroir. Ceci est à observer meme quand on se sert d'un miroir dont les bords sont inclinés à un petit angle, comme de 5°, bien que les bandes qui répondent à la portion des bords vers l'angle soient dilatées et éloignées dans une courbe hyperbolique, si elles sont formées de lumière blanche, ou que le miroir se trouve placé en travers du spectre. Pourtant, si la partie mince du miroir est placée dans les rayons violets, et les autres parties parallèles à l'axe du spectre, la partie rouge des bandes paraît un peu plus large et plus distante de l'ombre que la partie violette, la différence de flexibilité des rayons rouges étant plus considérable que l'effet produit par le peu de largeur du miroir.



Fig. 8.

10° Donc, il n'y a aucun doute sur cette propriété de la lumière. Les rayons de différente espèce sont non-seulement disposés en bandes de largeur différente par la force de flexion mais ils sont fléchis différemment; les angles de déflexion diffèrent dans les différents rayons, étant plus grands dans les moins réfrangibles, plus petits dans les plus réfrangibles; en un mot, leur déflexibilité est en raison inverse de leur réfrangibilité. Suivant le calcul ci-dessus donné de la proportion de 3 à 2, et supposant la déflexion moyenne telle que l'a donnée Newton (au moins telle qu'on la peut déduire de ses mesures), 3′ 32″, alors cet angle pour les rayons rouges sera de 4′ 14″, pour les violets de 2′ 49″. Ceci a rapport à la déflexion par un bord ou un autre corps seul. Les angles (c'est tout simple) sont beaucoup plus grands si deux bords agissent;

mais il n'y a pas lieu de croire que la proportion des angles est différente. Si la force fléchissante varie comme $\frac{1}{d^m}$ (d = distance du corps aux rayons), et si l'action sur les rouges est à l'action sur les violets comme 3 à 2, elle sera comme $\frac{3}{d^m}$ à $\frac{2}{d^m}$; par conséquent la distance ne signifie rien, bien que la différence de l'effet produit dans les rayons différents, sur la largeur des bandes et leur séparation entre elles, sera plus grande plus la distance des rayons aux bords est petite, cette différence étant comme $\frac{1}{d^m}$. Ainsi, cette différence est beaucoup plus facile à remarquer lorsque les deux bords agissent; et lorsqu'il n'y a qu'un seul bord, la différence est plus remarquable dans les bandes les plus près de l'ombre.

Nous avons fait observer que l'expérience newtonienne sur les largeurs des bandes ne conclut rien à cause de la propriété de lumière que nous venons de décrire, et qui avait échappé à l'illustre philosophe. It est probable que son erreur venait de ce qu'il avait aperçu à l'inspection simple que les bandes étaient plus larges dans les rayons rouges, et que, satisfait de cela, il n'appliquait ses mesures qu'à constater la proportion des largeurs. Mais il y a une autre portion de ses observations qui ne paraît pas appuyée par les phénomènes, je vieux dire la description des intervalles obscurs ou noirs lorsque les bandes sont formées par la lumière blanche. Il faut certainement la plus grande hésitation, même en osant exprimer un doute sur les récits d'un observateur si achevé. Cependant on peut concevoir que son attention n'ait pas été dirigée si rigoureusement au sujet du troisième livre qu'aux autres portions de son grand ouvrage. La preuve en est qu'il n'a pas remarqué les bandes internes ou de l'ombre du tout, bien que Grimaldi, qu'il cite, en ait fait mention. La raison est probablement qu'il avait fait ses expériences avec un cheveu; et les bandes internes ne sont facilement observées qu'avec un corps un peu plus large. Une aiguille $\frac{1}{20}^{e}$ de diamètre les

forme, mais pas si bien qu'une aiguille un peu plus large. Le cheveu dont s'est servi Newton n'avait que $\frac{1}{112}^{e}$ de largeur. Nous venons de voir aussi que ses mesures étaient peu concluantes sur les bandes du spectre, parce qu'il n'avait pas remarqué leur différent éloignement. Ne serait-il pas possible qu'il se fût trompé sur les intervalles noirs en regardant comme un espace obscur ou même noir le teint plus foncé des bandes là où le rouge de l'une touche au violet de l'autre? Que sais-je? Mais si l'on se donne la peine de regarder de près et avec grande attention ces bandes formées dans la lumière blanche, on sera convaincu que les couleurs se fondent, que le violet d'une bande se mêle avec le rouge de la bande voisine, et que ce qui d'abord avait paru ligne noire n'est que la confusion de ces deux couleurs. On a fait l'expérience avec toutes les mesures et toutes les proportions des observations newtoniennes : même grandeur de trou, $\frac{1}{42}$ de pouce anglais ; même distance de la fenêtre et du tableau au cheveu, 12 pieds l'une, 6 pouces l'autre ; et même largeur de cheveu. Les bandes ont été examinées à toute inclinaison du tableau, de la verticale à l'horizontale ; elles ont été reçues sur le verre dépoli, et, l'œil placé derrière le verre pour les recevoir directement, examinées avec une loupe ou à l'œil nu, et par plusieurs observateurs ; et bien que d'abord it ait paru qu'il y eût un intervalle noir, une ligne qui séparât les bandes, une inspection plus attentive et scrupuleuse a toujours fait voir que les bandes se fondaient l'une dans l'autre au point de leur rapprochement, là violet ou bleu de l'une se mêlant par un espace très-petit avec le rouge de l'autre. Lorsque le verre dépoli est placé très-près du corps, comme à moins d'un quart de pouce, on a plus de difficulté à apercevoir la fusion des bandes. Pourtant, si très-près du corps elles sont séparées, on ne peut pas facilement comprendre comment elles ne se croisent pas totalement et ne s'entrecoupent pas à une distance plus considérable. Il est évident que rien ne prouve que les observations n'ont pas été faites très-près du corps, parce que les mesures de Newton étaient reprises à une distance de 6 pouces et de 9 pieds.

Les lignes grises et noires au centre de l'ombre ne peuvent

jamais êtres confondues avec les bandes, et la séparation des bandes par ces lignes-là est complète.

Lorsque les bandes sont formées par la lumière homogène, sans nul doute les intervalles noirs paraissent plus certains d'exister, et il semble que lorsqu'il n'y a qu'une couleur elles doivent être séparées, à cause de la non-existence des autres couleurs dans la bande. Cependant on doit faire observer que Newton ne donne que la plus petit différence entre les distances des bandes rouges, par exemple, et des bandes de toutes couleurs formées par la lumière blanche. L'une est de $\frac{1}{37}$, l'autre de $\frac{1}{36}$ de pouce (différence de $\frac{1}{1406}$).

Il faut aussi faire remarquer que les bandes rouges, par exemple, examinées de près et sur un verre dépoli, l'œil derrière paraissent avoir les autres couleurs aussi. Le rouge domine, mais il y a du vert et du bleu ; bien que reçues sur le tableau, elles paraissent toutes rouges : cela vient évidemment de la présence de lumière blanche dispersée sans avoir passé par le prisme, mais aussi de la présence de lumière imparfaitement séparée par la réfraction. Cependant, comme les rayons autres que les rouges, par exemple, doivent être fléchis aux endroits différents de ceux où tombent les rouges, il paraît que ces endroits-là doivent être occupés par les autres couleurs, bien qu'ils paraissent noirs.

La même chose arrive avec le spectre prismatique lui-même. Faites un trou très-petit dans un écran, et laissez passer les rayons homogènes par ce trou et tomber sur le tableau. Derrière le trou placez un second prisme, vous verrez un petit spectre ayant le rouge, par exemple, plus abondant et à sa place, mais ayant aussi du jaune et du vert et du bleu à l'autre extrémité. Lorsque c'est le bleu ou violet qui passe par le trou le petit spectre a du vert et du rouge plus clairement que n'a de vert et de bleu le petit spectre formé par les rayons rouges.

Lorsque l'on examine les couleurs du spectre prismatique près du prisme, il n'y a que du blanc, excepté aux bords, qui sont colorés seulement d'une mince ligne de rouge d'un côté et de bleu de l'autre. Ces bords augmentent jusqu'à ce que les couleurs remplissent l'espace blanc dans la manière décrite

par Newton (*Opt.*, liv. I, part II, prop. viii); mais, à moins que l'angle réfractant du prisme ne soit très-grand, comme de 68° à 70°, le blanc continue à quelque distance du prisme. Comme cela, selon Newton, vient du mélange des diverses couleurs partant des différentes parties du prisme, il s'ensuit qu'un corps opaque, placé de manière à intercepter une partie des rayons avant leur passage à travers la ligne parallèle à l'axe du prisme, fera paraître des couleurs immédiatement derrière ce corps-là, mais non pas si le corps est placé verticalement à l'axe du prisme. Apparemment c'est pour cette raison que les bords fléchissants placés dans le blanc parallèlement à l'axe du spectre, et perpendiculaires à l'axe du prisme, forment des bandes de même espèce et même couleur que si les bords étaient placés dans les rayons blancs non réfractés, pourvu que les bandes soient reçues et examinées près des bords, et dans l'espace du spectre qui continue à être blanc. Regardées plus loin, elles deviennent colorées avec les teintes du spectre. Mais on ne comprend par trop comment sur l'explication newtonienne les rayons, une fois disposés par l'action des bords en bandes de couleurs tout à fait indépendantes de celles dont on suppose que la fusion produit le blanc du spectre, et étant devenus pinceaux de ces couleurs indépendantes, pourraient plus tard, et à une distance plus grande, devenir mêlés avec les couleurs qui avaient formé le blanc ; car les bords et les bandes qu'ils forment sont perpendiculaires aux rayons qui proviennent du prisme. Les bords (fig. 9, *a*, *b*) forment des bandes de couleurs entre *g* et *f*, différentes de celles *cf* et *dg*, qui s'entremêlent avant et jusqu'à E; au-delà de E, la fusion de *cf*, *dg* cesse. Mais comment est-ce que leur séparation dans ce sens-là agit ou influe sur leur séparation par *a*, *b*, dans un sens entièrement different? Si *a*, *b*, étaient placés en travers, de manière à intercepter *cf* ou *dg*, nul doute que l'effet produit ne fût de faire des couleurs dans la partie blanche du spectre. Mais cet effet serait produit de suite, passé *a*, *b*, et non pas à E

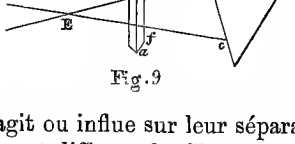


Fig. 9

seulement. Les rayons, ce semble, étant blancs à leur passage par les bords *a*, *b*, sont disposés en bandes par l'action de *a*, *b*, qui leur fait prendre une direction à un angle horizontal à *a*, *b*, c'est-à-dire que les bords décomposent le blanc en rouge, vert, bleu, par action latérale et horizontale ; et pourtant, par l'action verticale du prisme, ces mêmes couleurs sont changées passé E. Supposons qu'au lieu des bords *a*, *b*, un prisme fût placé verticalement, il devrait former un spectre avec le rouge le plus près de *a*, *b*, le violet de l'autre côté ou à l'autre bout du spectre, si toutefois l'angle réfractant du prisme est tourné vers *a*, *b*. Donc, si la même chose arrive à ce spectre qui arrive aux bandes, il s'ensuivrait que le rouge devrait être changé, au moins teint des couleurs qui, mêlées ensemble entre *c* et *f*, *d* et *g*, sont séparées passé E, ce qui évidemment n'arrive pas.

Cependant la grande diversité de l'action de flexion et de réfraction doit toujours nous être présente, et il n'y a rien dans ces phénomènes de plus remarquable. Lorsque la lumière homogène passe par les bords, parallèlement et non pas divergente, elle est disposée en bandes non-seulement à distances différentes de l'axe du spectre, mais de largeur diverse. Le trait ou pinceau est distendu. Lorsque la lumière est réfractée par un second prisme placé verticalement au premier ou parallèle à l'axe du spectre, il est réfracté aux diverses distances de l'axe, le violet le plus éloigné, le rouge le plus près. En cela il y a grande ressemblance avec les phénomènes de flexion si ce n'est que les rayons les moins réfractés sont le plus fléchis. Mais là cesse l'analogie des deux opérations ; car il n'y a pas dans la réfraction par le second prisme la plus petite distension ou dilatation du pinceau, comme il pourrait y avoir si le second prisme était placé horizontalement ou à travers le spectre ; car alors, bien que les rayons, tous de la même couleur, ne puissent pas être distendus, cependant un trait composé de plusieurs couleurs pourrait être distendu. Mais dans la flexion c'est différent. Les bords placés parallèlement à l'axe du spectre forment des bandes autant que s'ils étaient placés à travers le spectre, et les pinceaux sont distendus latéralement, quand même les rayons qui les composent sont exactement de

même couleur. It est vrai que les bandes sont plus larges lorsque les bords sont placés parallèlement au prisme et en travers du spectre, à cause de la différente flexion des rayons différents; mais cette augmentation relative n'est pas très-considérable, parce que les rayons près des bords (oranges, par exemple) ne sont pas autant fléchis que les rouges plus loin, cette différence étant une compensation de la plus grande distance de ceux-ci ; et ainsi la dispersion est plus petite qu'elle ne serait, à cause de la proximité des uns et de la distance des autres.

Ces deux propriétés,—la différente distension (ou dispersion) des différents rayons indiquée par la différente largeur des bandes, et la différente flexibilité des rayons indiquée par la différente distance des bandes,—voyons comment on peut les expliquer, et si elles sont indépendantes l'une de l'autre, ou si elles peuvent être ramenées au même principe.

Newton, pour l'explication de la première propriété (la seule qu'il ait remarquée), a donné l'hypothèse que l'action des corps s'étend plus loin sur les rayons moins réfrangibles ; et il paraît penser qu'à la même distance l'action est la même, mais que cette action, plus près sur les uns, égale l'action plus loin sur les autres. Cela explique certainement la différence de largeur, mais non pas le différent éloignement des bandes. Pour expliquer cela, il faut que l'action ne soit pas seulement égale à une plus grande distance, mais qu'elle soit plus forte à la même distance.

Il y a pourtant une objection à faire à la théorie newtonienne. C'est que l'action ne cesse pas avec la première bande; il y en a une suite d'autres, toutes produites par la continuation de la même action, diminuant avec la distance. Ainsi la théorie n'a pas d'application, à moins qu'on n'ajoute une hypothèse encore, savoir : Qu'il y a une suite de sphères d'action, chacune répondant à une bande, et que dans chaque sphère l'action s'étend plus loin sur les rayons les moins réfrangibles. Mais il faut ajouter encore un hypothèse, ce me semble, pour expliquer le plus grand éloignement de bandes formées par ceux-ci. Il faut que la sphère, ou plutôt les sphères, d'action

commencent, pour les diverses couleurs, à différentes distances du corps fléchissant. En faveur de cette hypothèse, ou pourrait faire remarquer la bande ou ligne assez brilliante de blanc touchant à l'ombre, et entre l'ombre et la première bande colorée. Cette ligne blanche a toujours paru difficile à expliquer ; mais je ne suis pas d'avis que l'explication dépende uniquement de ce que l'on vient de dire sur le commencement des sphères d'action. Il faut se rappeler que la déflexion commence là où l'inflexion cesse. Ainsi les rayons qui passent le plus près du corps sont exposés à toutes les deux actions, et ne peuvent pas être décomposés plus qu'ils ne le sont en passant par deux prismes dont les angles réfractants sont placés en sens inverse. Dans ce cas-là, il n'y a que le blanc qui sorte ; ou si la lumière est homogène, il n'y a pas de changement dans le cours des rayons. Même chose pour l'action des bords. Une troisième hypothèse me paraît mériter notre attention, d'autant plus qu'elle pourrait peut-être fournir l'explication de toutes les deux propriétés, et que les règles de philosopher défendent la multiplication de causes ou de principes. Il se peut que la proportion de l'action, à la distance du corps, varie dans les différents rayons ; que le long du spectre dans l'équation $y = \frac{a}{\frac{1}{x^z}}$ (y = force flectrice ; x = distance du bord ou corps ; z = l'axe, ou plutôt les portions successives de l'axe du spectre ; z = AP, x = Pq ; AB = l'axe) (fig. 10). Ainsi z varie dans les couleurs, ou le long de l'axe, et diffère pour tous les rayons de l'extrême rouge à l'extrême violet. Nous avons donc une équation exponentielle, mais peu compliquée.

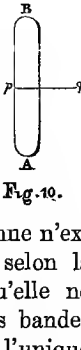


Fig. 10.

Nous avons fait observer que l'hypothèse newtonienne n'explique aucunement la distance variante des bandes selon la réfrangibilité des rayons. Aussi faut-il convenir qu'elle ne peut du tout expliquer les couleurs prismatiques des bandes formées par la lumière blanche. Supposons que l'unique différence des rayons fût que l'action du corps fléchissant

s'étendît plus loin sur les rouges et moins sur les autres successivement; le résultat serait que les rayons rouges seraient disposés sur un espace, comme bande, R V, et plus large de *o* R que les espaces qu'occupent les autres; les oranges seraient disposés sur l'espace V *o*; les jaunes, sur l'espace V *j*; et ainsi des autres, de manière que la seule partie qui serait d'une couleur simple et unique, c'est *o* R; tandis que toutes les autres seraient teintes d'un mélange de couleurs, *jo*, rouge et orange; V' *j*, rouge, orange et jaune; *b* V', rouge, orange, jaune et vert; *ib*, ces couleurs avec le bleu; *vi*, ces couleurs avec l'indigo; et V *v*, toutes les couleurs ou blanc. Rien ne peut être plus différent de l'apparence des bandes; les teints saillants sont rouge, vert et bleu. Or, selon la théorie, le vert serait mêlé avec le rouge, l'orange et le jaune, et le bleu avec toutes ces couleurs; et, finalement, l'espace qui devait être violet serait blanc.

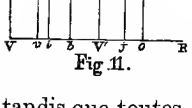


Fig 11.

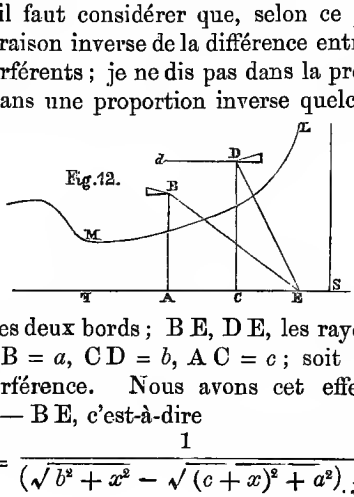
La différente étendue de l'action n'explique donc pas du tout les couleurs des bandes. Rien ne les explique, que la différente flexion des différents rayons, de manière à faire occuper aux couleurs des places différemment éloignées de l'ombre. Mais cette différente flexion donne l'explication très-facilement. Il n'y a que la différente largeur des bandes de couleurs différentes qui donne le moindre embarras, et cela n'est pas considérable. Il s'ensuivrait de cette différence que la partie rouge du spectre de flexion (c'est-à-dire de la bande formée par la lumière blanche) devrait être plus large que les autres parties, et la violette la plus mince de toutes. Mais la rouge et l'orange se confondent, et font une partie matérielle de la bande; le violet, l'indigo et le bleu de même paraissent bleus; le vert et le jaune passent pour verts; et ainsi les couleurs paraissent plutôt rouge, vert et bleu, qu'en plus grand nombre.

Nous avons parlé, mais peu, des bandes internes. Evidemment les rayons qui les forment viennent des côtés opposés du corps fléchissant, et se croisent ou au moins se rencontrent à un point plus ou moins distant du corps, selon que ce corps est plus ou moins mince. Qu'ils se croisent ou se touchent, paraît

d'abord par l'expérience fondamentale d'interférence, l'obstruction des rayons d'un côté et l'effacement des bandes du côté opposé. Mais cela paraît aussi, si les bandes sont examinées près du corps, en les recevant sur un verre dépoli, l'œil placé derrière. Il n'y a point de bandes si le verre n'est pas à une certaine distance ; plus près, il n'y a que l'ombre parfaitement noire. La détermination de ce point, où commencent à paraître les bandes internes, nous donne le moyen de calculer l'angle d'inflexion, le demi-diamètre de l'aiguille ou un autre corps mince étant le sinus de cet angle, et la distance du corps son cosinus. Je n'ai jamais pu fixer cet angle à moins de 20′ ; ce qui démontre combien la force d'inflexion diffère de celle de déflexion, l'angle de déflexion étant, comme nous l'avons vu plus haut, six fois moins grand. Des observations m'ont fait conclure que l'angle d'inflexion des rayons rouges est de 27′ à 30′, et il est probable que cet angle pour les violets est de 18′ à 20′ au moins, si la proportion de 3′ à 2′ se conserve pour les bandes internes comme pour les externes.

Pour pouvoir décider la question, savoir, si les phénomènes de flexion (diffraction) peuvent être ramenés au principe de l'interférence, il faut considérer que, selon ce principe, l'effet produit est en raison inverse de la différence entre les longueurs des rayons interférents ; je ne dis pas dans la proportion simple inverse, mais dans une proportion inverse quelconque. Soient

Fig. 12.



B, D (fig. 12), les deux bords ; B E, D E, les rayons interférents à E ; mettez A B = a, C D = b, A C = c ; soit C E = x ; y = l'effet de l'interférence. Nous avons cet effet-là en raison inverse de D E — B E, c'est-à-dire

$$y = \frac{1}{(\sqrt{b^2 + x^2} - \sqrt{(c + x)^2 + a^2})}.$$

Or, cette courbe doit avoir une asymptote, quelle que soit la valeur des constantes de l'équation, et quelle que soit la valeur de m; c'est-à-dire dans toutes les positions des bordes, et quel que soit l'ordre de la courbe. C'est-à-dire que, quelle que soit la loi d'interférence, pourvu que l'interférence agisse en raison inverse quelconque à la différence des longueurs des rayons, on peut toujours trouver un point S, auquel D E = B E ou $\sqrt{b^2 + x^2} = \sqrt{(c + x)^2 + a^2}$. Ce point-là se trouve où $x = \frac{b^2 - c^2 - a^2}{2c}$. Donc la valeur de y augmente entre A et ce point S, où elle devient infinie. Donc les bandes doivent augmenter en largeur et en éloignement l'une de l'autre, dès le point A vers le point S. Mais au contraire elles diminuent en largeur et en distance. La plus large est la plus près de A; les autres diminuent constamment jusqu'à ce qu'elles disparaissent; et là où il y a des intervalles entre elles, comme dans la lumière homogène, ces intervalles sont plus grands entre les bandes le plus près de A, et vont en diminuant vers S, toute comme les bandes elles-mêmes. Plus le bord B est près de D dans le sens C A ou Dd, et plus éloigné sera le point S. Cela paraît non-seulement par la valeur de x ci-dessus, mais aussi par la raison géométrique de la solution du problème de trouver le point où deux lignes infléchies de deux points sur une troisième ligne sont égales. Ainsi, plus les bords sont près l'un de l'autre dans le sens d D, et plus les bandes devraient être minces et rapprochées l'une de l'autre; ce qui est diamétralement contraire aux phénomènes.

Jusqu'ici nous n'avons regardé que le cours de la courbe de A à S, en le comparant avec les phénomènes de ce côté-là de A B. Maintenant considérons la courbe du côté opposé de A vers F. Elle approche de l'axe durant une portion de son cours, et ne commence à s'en éloigner qu'à M, là où il y a un point de rebroussement. Donc, entre A et F (l'abscisse pour le point de rebroussement), les ordonnées diminuent, et ne commencent à augmenter que passé F. Pour trouver F,

il faut trouver y en termes de x dans l'équation $\frac{d^2y}{dx^2} = 0$.

Mais l'opération devient embarrassante, même accablante, le dénominateur de la première différentielle de y ayant 16 facteurs multipliés par la racine carrée d'une fonction de 4 facteurs, et puis une quantité de 30 facteurs à soustraire ; et le numérateur est même plus compliqué, et puis le tout doit être différencié pour avoir d^2y. Mais on peut trouver la valeur approximative de $-x$; et si l'on prend les proportions de A C, A B et D C, les distances auxquelles l'expérience se fait commodément, $a = 80$; $b = 90$, et $c = 1^m$; le point S est à $849\frac{1}{2}$ de A; et F C = 9.9, ou A P = 8.9; car à $x = -8$, $y = \frac{1}{10.039}$; à $x = -9$, $y = \frac{1}{10.049}$; à $x = -9.5$, $y = \frac{1}{10.05}$; $x = -9.9$, $y = \frac{1}{10.04}$; $x = -10$, $y = \frac{1}{10.049}$; $x = -12$, $y = \frac{1}{10.044}$, et à $x = -15$, $y = \frac{1}{10.026}$.

Donc il est clair que si les phénomènes étaient causés par l'interférence, les bandes devraient diminuer tant en largeur qu'en distance l'une de l'autre, de A jusqu'à F ; car y va en diminuant entre ces deux points. Mais au contraire les bandes augmentent en largeur et en distance, non-seulement passé F, mais sur toute la route de A à F.

Il faut faire remarquer que ces phénomènes sont tous observés, et en effet ne peuvent être observés qu'assez près des lignes A B, C D. Car d'après les proportions ci-dessus, et qui sont celles des expériences qu'on a réellement faites, les bandes d'un côté (celles de déflexion après l'inflexion) ne sont visibles que dans un espace de 3 à 4 mill. ; et les bandes augmentent de l'autre côté, celles d'inflexion après déflexion, dans un espace plus considérable, mais seulement de 6 à 7 mill. Mais les premières, qui devraient augmenter jusqu'à S, vont constamment en diminuant jusqu'à ce qu'elles cessent d'être visibles, tandis que les secondes, qui devraient

diminuer jusqu'à un certain point F, vont en augmentant toujours, et ne diminuent jamais.

Il faut aussi faire attention à ce que l'on a pris la courbe dans la supposition que $m = 1$, ou que l'action d'interférence est en raison inverse simple de la différence des longueurs; mais le raisonnement est le même, quelle que soit la valeur qu'on donne à m. Soit l'action en raison inverse des carrés ou $m = 2$, ou des racines carrées, ou $m = \frac{1}{2}$, on trouvera que la courbe est de la même forme en ce qui regarde cette portion dont il est question. Les distances des points S, F sont les mêmes. Les courbes sont d'ordres différents, et leurs autres branches varient de beaucoup de celles de la courbe que l'on vient d'examiner. Mais en ce qui regarde la branche dont il s'agit, il n'y a pas de différence.*

Si l'on regarde les bandes internes ou de l'ombre, le principe d'interférence est difficile à appliquer, mais l'application n'est pas impossible. Soit a le diamètre de l'aiguille; b, la distance du tableau où les bandes sont reçues; x, la distance de l'extrémité du diamètre a, vers son centre; l'équation est $y = \frac{1}{(\sqrt{(a-x)^2 + b^2} - \sqrt{b^2 + x^2})^m}$; et ici, comme dans l'autre cas, nous avons une asymptote, savoir, quand $x = \frac{a}{2}$, et les ordonnées augmentent de $x = 0$ jusqu'à $x = \frac{a}{2}$; et les phénomènes s'accordent avec la théorie à un certain point, car les bandes augmentent très-faiblement

* Si $m = 1$, la courbe est du huitième ordre.
Si $m = -1$, elle est du quatrième ordre.
Si $m = 5$, elle est du sixième ordre.
Si $m = \frac{1}{2}$, elle est du douzième ordre.

Mais la forme ne varie pas beaucoup. Il va sans dire que lorsque $m = -1$, il n'y a pas d'asymptote. Si la proportion est, non pas de la différence des rayons, mais de leur carré, hypothèse presque impossible, la courbe est une hyperbole conique, $y = \frac{1}{b^2 - a^2 - c^2 - 2cx}$; et un porisme assez curieux se rapporte à cette propriété de la courbe.

jusqu'à l'axe de l'ombre, et si peu que plusieurs observateurs ont affirmé qu'elles sont toutes de la même largeur. Au centre de l'axe, pourtant, il y a un espace gris, manifestement plus large que les bandes ; et il y a deux intervalles d'un noir foncé entre cet espace gris et les bandes colorées. Ces deux intervalles noirs sont aussi plus larges que les bandes, et que les autres intervalles noirs. Mais la théorie indique une augmentation de largeur beaucoup plus considérable. Prenez $m = 1$; $a = \frac{1}{20}{}^{c}\, b = 2\frac{1}{2}$, 5, $7\frac{1}{2}$, 15, 75, 100 successivement, nous aurons la proportion de la valeur de y lorsque $x = 0$, et lorsque $x = 2\frac{3}{4}$ du demi-diamètre (ou de $\frac{1}{40}$), c'est-à-dire très-près du centre, comme 1 : 12. Ainsi, les bandes près du centre doivent être 12 fois plus larges qu'à l'extrémité de l'ombre. Mais, même en comptant les bandes noires et grises centrales, elles ne sont jamais près du double. Si $m = 2$, ou plus, la différence est beaucoup plus grande. Même en prenant $m = \frac{1}{2}$ ou $\frac{1}{3}$ (racine carrée ou cube), la disproportion est beaucoup trop grande. Si $m = \frac{1}{4}$, elle est encore considérable, comme 47286 : 87263. Donc, sans être impossible, il est difficile de ramener les bandes internes au principe d'interférence. Pour ce qui regarde les bandes extérieures, cela devient impossible. Il y a là certainement opposition complète des phénomènes à la théorie.

La théorie ou hypothèse de M. Fresnel, dont j'ai parlé dans mon Mémoire de l'année passée, est d'une grande importance, je veux dire la proposition que les phénomènes de flexion (diffraction selon lui) "dépendent uniquement de la largeur de l'ouverture par laquelle la lumière est introduite." (*Mém. de l'Inst.*, 1821, 1822, p. 372.) Si cela est vrai, toute l'influence des corps fléchissants disparaît, et tout est réduit à la question de la largeur de l'ouverture.

La preuve la plus directe du contraire est aussi la moins facile à obtenir par les expériences ; mais on peut l'avoir. C'est la mesure de l'ouverture lorsque les bords sont placés directement vis-à-vis l'un de l'autre. La largeur des bandes n'est pas en raison inverse de la largeur de l'ouverture.—La *seconde* preuve est de placer les bords l'un après l'autre sur

une ligne rigoureusement horizontale, et parallèle aux rayons parcourant horizontalement la chambre. Les bandes et leurs distances des rayons directs, qui ne sont que de $\frac{1}{5}$ de mill. et même moins, si les bords sont distants l'un de l'autre horizontalement de 10 cent., ont la largeur et l'éloignement de 2 mill. lorsque la distance horizontale des bords est d'un cent.; et les bandes ont la largeur et l'éloignement de 10 cent., lorsque les bords ne sont que $\frac{1}{5}$ mill. distants l'un de l'autre. Mais la distance verticale des bords l'un de l'autre reste la même, elle est d'un mill.: c'est-à dire l'ouverture reste la même, tandis que la distance horizontale variée a entièrement changé la largeur et l'éloignement des bandes; démonstration conclusive que la largeur de l'ouverture ne décide pas de celle des bandes et de leur éloignement des rayons directs. Pourtant cette expérience exige le parallélisme rigoureux de la ligne ou barre sur laquelle les bords sont placés. Ainsi je donne encore une preuve qui paraît décisive sans que l'exact parallélisme soit nécessaire; et par conséquent cette troisième expérience est facile à faire.

Placez les bords dans un pinceau, n'importe de quelle inclinaison ni à quel angle les bords le rencontrent; ils feront des bandes plus ou moins larges, plus ou moins éloignées des rayons directs, en proportion inverse de la distance des bords l'un derrière l'autre dans le sens du pinceau. Soit cette distance de 10 cent., et faites que le bord le plus près du trou qui admet la lumière dans la chambre se porte de plus en plus dans le pinceau, jusqu'à ce que le passage des rayons entre les deux bords soit fermé, et qu'il n'en passe plus. Remarquez bien les bandes avant qu'elles disparaissent, et vous verrez qu'à cette distance des bords ces bandes n'atteignent jamais qu'une largeur très-petite, même lorsqu'elles sont évanouissantes. Rapprochez les bords, et retirez du pinceau celui qui est le plus près du trou, jusqu'à ce que les rayons puissent passer entre les deux bords (ces bords sont maintenant à une distance l'un de l'autre d'un cent., selon le cours du pinceau); et vous verrez que quand même la distance verticale des bords n'est pas très-petite, il y a des bandes considérables. Faites entrer le bord dans le

pinceau jusqu'à ce que les bandes soient d'un mill. ou mill. et demi de largeur, et que la distance des bords selon le cours du pinceau ne soit que d'un cent. ou $\frac{2}{3}$ de cent. ; puis placez le second bord à une distance de 10 cent. ou de 20 cent., faites-le entrer dans le pinceau, et vous trouverez que quand même le bord est placé dans le pinceau de manière à faire intercepter tous les rayons, au moment de l'évanouissement des bandes elles ne sont jamais de la largeur dont elles étaient lorsque les bords furent placés l'un près de l'autre, pas même du dixième de cette largeur. Donc, à des distances même peu considérables des bords l'un derrière l'autre, il n'y a pas de petitesse d'ouverture (ou distance verticale de ces bords) qui puisse former des bandes tant soit peu larges.—J'ai vu ceux qui penchaient du côté de l'hypothèse de l'ouverture être convaincus tout de suite de leur erreur, en voyant qu'à plusieurs assez petites distances des bords l'un derrière l'autre, on peut varier à l'infini leur distance verticale, c'est-à-dire l'ouverture, sans qu'aucune diminution de cette ouverture puisse augmenter la largeur ni l'éloignement des bandes considérablement.

Qu'il me soit permis, avant de conclure, de faire observer que Newton, dans un passage remarquable de son troisième livre, paraît mais assez obscurément, s'être douté d'une propriété des rayons telle que je l'ai décrite sous le nom de *disposition* dans mon Mémoire de 1849.* En parlant des deux bords ou tranchants de couteau, il dit que le couteau le plus près de chaque rayon détermine le cours que prendra ce rayon, et que l'autre augmente la flexion. Or l'autre, c'est le bord opposé ; et ceci me paraît approcher de très-près de ma théorie.

SUPPLÉMENT.

Dans mon dernier Mémoire,† en donnant les preuves de la différente flexibilité des rayons homogènes, je me suis

* Un résumé des expériences et des conclusions qu'on en a tirées, a été lu plus tard à la Société royale. (Voir 'Phil. Trans.' 1850, part II.)

† Il précède ce Supplément.

exprimé avec quelque hésitation sur le cours rectiligne, que j'attribuais aux bandes formées par la lumière du spectre prismatique. Les expériences avec deux bords très-près l'un de l'autre, placés dans les rayons parallèlement à l'axe du spectre, ont paru donner des bandes rectilignes, ou à très-peu près. La distance des bords dans ces expériences était en général de $\frac{1}{5}$ de millimètre, et rarement de moins de $\frac{1}{10}$. J'ai fait depuis des expériences avec un réseau de lignes gravées sur verre, et distantes entre elles de $\frac{1}{25}$ et aussi de $\frac{1}{50}$ de millimètre; et j'ai été surpris de trouver qu'à la première de ces distances, mais bien plus à la seconde, les bandes étaient courbées, et d'une assez grande courbure. Cette forme de l'expérience est assez commode, parce que, quoique deux de ces lignes gravées, seules, donnent des bandes peu distinctes, six ou plus rendent les bandes très-brillantes, en faisant tomber sur le tableau (ou sur la rétine lorsqu'on regarde le phénomène par voie de vision directe) les bandes formées par plusieurs lignes, ou paires de lignes, du réseau.

La fig. 1 donne les bandes formées sur le tableau, lorsque le réseau est placé dans les rayons du spectre prismatique.



Fig 1

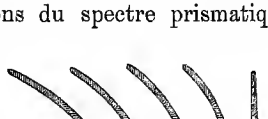
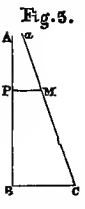


Fig. 2.

La fig. 2 donne les bandes vues par vision directe, le réseau étant placé près de l'œil et derrière le prisme, et le prisme placé entre le réseau et un assez petit disque de lumière solaire jetée sur un tableau blanc, près du petit trou par lequel la lumière entre dans la chambre obscure.

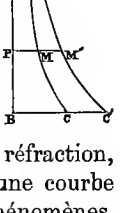
Fig. 3.



Soit A B, fig. 3, l'axe du spectre, A = violet, B = rouge. Si la force flectriee (ou l'influence, quelle qu'elle puisse être, par laquelle les bandes sont formées) augmente en raison directe de la distance de P à A, cette force agissante en lignes parallèles (et perpendiculairement aux bords fléchissants),

P M = y, A P = x, nous avons l'équation $y = \frac{x}{m}$, ligne droite, et d M C est rectiligne. Evidemment donc si d M C est curviligne, la force P M (fig. 4) n'est pas en raison simple de A P, c'est-à-dire en raison simple inverse de la réfrangibilité; et l'équation de d M C est $y = \frac{x^u}{m}$, et celle de d' M' C' est $y' = \frac{x'^u}{m'}$. Mais nous avons pris la réfrangibilité comme une fonction du sinus de réfraction soustrait de la constante A B. Si l'on prend la réfrangibilité en raison du sinus de réfraction, toute proportion inverse de la réfrangibilité donne une courbe hyperbolique qui ne peut être d'accord avec les phénomènes, excepté en supposant le centre de l'hyperbole assez éloigné de l'origine du spectre (le rouge); et quoique dans ce cas la ligne serait à peu près droite, la force ne serait pas en raison inverse de la réfrangibilité, c'est-à-dire du sinus de l'angle de réfraction, mais de la différence entre ce sinus et une autre ligne. Mais si l'on doit faire cette supposition, on pourrait également supposer la proportion directe de la différence en partant du violet, ce qui donnerait une ligne rigoureusement droite.

Fig. 4.

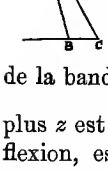


Mes mesures de la distance des courbes à l'axe du spectre, c'est-à-dire des lignes P M, P M', B C, B C', m'ont donné lieu à supposer que $n = 3$, et que la courbe est parabolique. Elles ne s'accordent pas avec une courbe hyperbolique, A B = 18, B C = 13, B C' = 15. Il faut pourtant faire observer que la courbe $y = \frac{x^3}{m}$ ne paraît pas d'accord avec la courbure des bandes. Car le rayon de courbure étant $\frac{(q\,x^4 + m^2)^{\frac{3}{2}}}{b\,m^2\,x}$, ce rayon paraît être plus grand pour d' M' C' que pour d M C, excepté très-près de d et d'. Car, égalant les quantités $\frac{(q\,x^4 + m^2)^{\frac{3}{2}}}{m^2}$ et

$\frac{(q\,x^4 + m'^2)^{\frac{3}{2}}}{m'^2}$ pour trouver le point M ou M', ou P, où les deux courbes ont la même courbure, on trouve $x = 7\frac{1}{2}$ à peu près.

Dans mon dernier Mémoire (qui précède ce supplément), j'ai suggéré que l'hypothèse d'une force flectrice variant avec la différence de réfrangibilité, $y = \frac{a}{x^{\frac{1}{z}}}$ (x = distance du rayon au point du bord fléchissant, z = distance du point à l'extrémité rouge du spectre), pourrait expliquer la différence de la largeur des bandes formées par les rayons de différentes couleurs ; et qu'ainsi la différente flexibilité expliquerait les deux phénomènes, le différent éloignement des bandes et leur différente largeur.

Fig. 5.



Soit A', fig. 5, un point des bords, sa distance de l'extrême rouge = z. A P = x, la distance du rayon au point A, x' = la distance d'un rayon plus éloigné du point A. Si la flexion est en raison inverse de la distance x, x', la différence B C des sinus des angles de flexion A P B, A P'C, donnera la largeur de la bande. Mais nous supposons que la force $y = \frac{a}{x^{\frac{1}{z}}}$. Ainsi, plus z est grand, et plus B C, différence des sinus des angles de flexion, est grand. Soit A P = 2, A P' = 3 ; et près de l'extrême rouge du spectre $z = 2$, $y = \frac{a}{\sqrt{2}}$ et $y' = \frac{a}{\sqrt{3}}$, et $\mathrm{B\,D} = \frac{(\sqrt{3} - \sqrt{2})}{\sqrt{6}} \times a$. Mais plus loin du rouge, $z = 3$; et $y = \frac{a}{\sqrt[3]{2}}$ et $y' = \frac{a}{\sqrt[3]{3}}$ et $\mathrm{BC} = \frac{(\sqrt[3]{3} - \sqrt[3]{2})\,a}{\sqrt[3]{6}} - a\frac{(\sqrt[3]{3} - \sqrt[3]{2})\,a}{\sqrt[3]{6}} < \frac{(\sqrt[3]{3} - \sqrt[3]{2})\,a}{\sqrt[3]{6}}$, et la bande violette est moins large que la bande rouge.*

* This tract is from 'Mém. de l'Institut' for 1854.

IX.

ON FORCES OF ATTRACTION TO SEVERAL CENTRES.

FORCES INVERSELY AS THE DISTANCE.

1. It is to be lamented that Sir I. Newton did not treat the problem of forces directed to more fixed points than one, as to two such points, either in the same or different planes from the body acted on. This is the fundamental point in considering disturbing forces when the centres are not fixed, which makes the problem more complicated and difficult. It is, however, sufficiently so even where the centres are fixed.

2. That the subject must have attracted his attention there can be no doubt. He had gone so much into the more difficult inquiries respecting disturbing forces that he must have fully considered the somewhat simpler, what may be termed the fundamental, case of fixed centres. Indeed, a paper communicated to the Royal Society in 1769 (*Phil. Trans.* p. 74) contains a demonstration by W. Jones, an intimate friend of Newton, of a proposition on this subject, which Machin had immediately after Sir Isaac's death given to the translator of the Principia. Machin had observed on the want of some investigation of the motion of forces directed to two centres, as required to explain the motions of planet and satellite, which gravitate to different centres, in a word the problem of the Three Bodies. The proposition of Machin and Jones goes but a very little way to supply the defect complained of. It is confined to the case of the line joining the two centres being in different planes from the line of

projection; it is that the triangle formed by the radii vectores and the line joining the two centres or fixed points, describes equal solids in equal times round that line; and the demonstration is similar to that of the first proposition, of equal areas in equal times when a single force is directed to one centre. It seems reasonable to conclude, that Newton had, upon full consideration, found the full investigation of the subject beyond the powers of the calculus as it then existed. It is at least certain that, though he might have mastered it, he never could have delivered his results synthetically as in the Principia.

3. The solutions on disturbing forces generally consider one force as acting in the one direction, that of the radius vector, and another in a line perpendicular to that radius vector. Thus Clairaut (*Mém. Acad.* 1748, p. 435) gives these equations $r\,d^2v + 2\,dr\,dv = \Pi\,dx^2$

$$r\,dv^2 - d^2r \quad = \Sigma\,dx^2;$$

r being the radius vector, v its angle with the axis, dx the differential of the time, Π the force to the centre, Σ the disturbing force. So D'Alembert (*Mém. Acad.* 1745, p. 365) takes the same course, and obtains an equation to the orbit in question, depending on the integration of $\int \Pi\,dz$, Π being the disturbing force acting in a line perpendicular to the radius vector, and z the circular arc described with a radius equal to the distance between the centre of force and the vertex of the orbit. This assumes, however, that the orbit is itself nearly circular.

4. If P = distance of E (Earth) from Moon (M)'s quadrature, s = sin. angle of rad. vec. r with the perpendicular to a, the distance of E from S, the Sun; v = velocity of M; then $v\,dv = \frac{r\,dP}{P} + \frac{3\,P^2\,m\,n\,s\,ds}{a^3}$, supposing the motion of M to be almost uniform. Here one of the forces acting on M is directed towards E, and is $= \frac{E+M}{M\,E^2} + \frac{S \times M\,E}{S\,M^3}$; the other force is in a line parallel to S E or a, and is =

$\frac{S \times SE}{SM^3} - \frac{S}{SE^2}$. It was in consequence of this investigation that Clairaut for some time announced, as did also Euler and D'Alembert, that there was a material error in the Newtonian theory of the Moon's motion. The error, which afterwards was found to arise from their having omitted the consideration of certain quantities, was acknowledged by Clairaut three years later (*Mém. Acad.* 1748, pp. 421, 434), but no one can read that paper without feeling that the acknowledgment was too coldly made, after he had gone so far as to suppose that the whole Newtonian doctrine was overthrown, and to propose a new law of $\frac{1}{r^2} + \frac{1}{r^4}$, the whole of this arising from his own error. It is to be remarked, however, that the investigation of 1745 was in all respects most accurately conducted, and must have led to the same result as in 1748 but for the supposition that certain quantities might safely be neglected. Even in 1745, Clairaut, upon Newton's assumption of the excentricity of M being nothing, comes to his conclusion that the proportion of the axes is as 69 to 70.

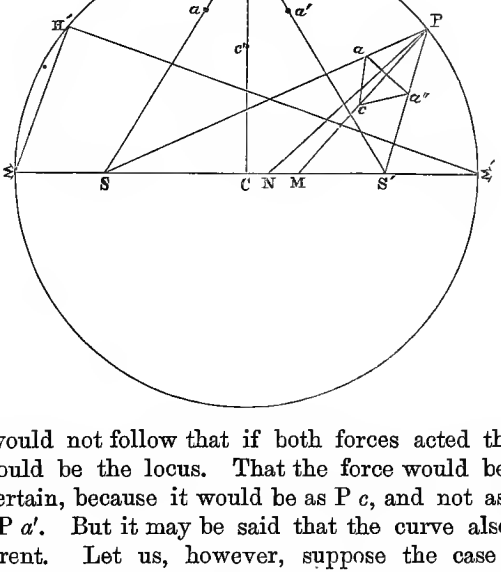
4. Legendre treats the subject very fully, as far as regards two centres, and also confining himself to the forces being inversely as the square of the distance (*Exercises de Calcul. Integral.* part iv. sect. 2). He deduces from his analysis several theorems, two of which he regards as very remarkable. The first apparently strikes him in this light, because it shows the same orbit to be produced by the combined action of the two forces directed towards two foci, as either force would produce acting on the body, and directed to one of the foci. If V is the velocity at the vertex of the ellipse which would make the body describe that curve when acted upon by the force directed to one focus, v the velocity at the same point which would make the body describe the ellipse when acted upon by the other force directed to the other focus; then if the two forces act together upon the body, and

I is the initial velocity, or velocity of projection, it will describe the same ellipse, provided $I^2 = V^2 + v^2$.

5. The other theorem follows from his integration which gives the expression for the time. It is that if two equal forces act upon the body directed to the two foci, and the masses of the attracting bodies consequently are equal, the revolving body will describe the ellipse in a shorter periodic time, will move more swiftly, than if the whole mass were placed in one focus and acted from thence upon the revolving body. He takes the example of the tangent of the orbit with the axis making an angle of 30°, and finds the periodic time shorter in the proportion of nearly 78 to 100 when the attracting mass is divided into two, one acting in each focus, than when both combined act from one focus.

6. What renders this problem of more centres than one so difficult, is that the resultants of the forces pass through different points, and that they vary by a law which differs in each case, as the locus of their extremities is a different curve. Take the least complicated, but still full of difficulty, that of two fixed points as the centres of force, and take the instances in nature of the forces being inversely as the square of the distance; the radius vector to one point being r, the force $\frac{m}{r^2}$; to the other point the radius vector q, the force $\frac{m}{q^2}$. Now the force which acts on the body being the resultant of these two, and these forces not being as r and q, the diagonal does not pass through the middle point of the line joining the two centres; except in the single point of the orbit where $r = q$, and even then it may not reach that line, for it is $\frac{\sqrt{2} \cdot m}{r^2}$. At every other point it runs in a different direction. Let S and S′ be the two fixed points; S P = r, and S′ P = q. Then P a being taken = $\frac{m}{r^2}$, and P a' = $\frac{m}{q^2}$, the resultant at that point

P bisects $a\,a'$ and is P c, and produced, P M cutting the axis. From hence may be seen how complicated would be the analysis, how next to impossible the geometrical construction of the locus of P, by referring the lines P M to S S′ as an axis. We know indeed that one of the forces $\frac{m}{r^2}$ or $\frac{m}{q^2}$ acting towards S or S′, the locus of P is an ellipse;



but it would not follow that if both forces acted the same curve would be the locus. That the force would be different is certain, because it would be as P c, and not as either P a or P a'. But it may be said that the curve also would be different. Let us, however, suppose the case of the curve, whatever it be, cutting the axis S S′ produced at Σ and Σ', points equally distant from S and S′, so that $S\Sigma = S'\Sigma'$; also that the angle and the initial velocity of projection from Σ and Σ' is the same, and further that the attraction as the mass is the same from S and S′, or that the mass of the body in S and in S′ is the same; then it

seems impossible to avoid the conclusion that an ellipse, and the same ellipse, must be described; because one of the forces alone acting from S, as $\frac{1}{r^2}$, would give the ellipse passing through Σ and Σ'; and the other force alone acting from S, as $\frac{1}{q^2}$, would give the ellipse passing through the same points Σ and Σ'; and the initial velocity * and angle of projection would prevent any difference in the length of the conjugate axis; and in the middle point answering to the centre C, the equality of r and q and of Pa and Pa' would make the diagonal Pc coincide with the conjugate axis. But a further combination of forces may be supposed in this case; two forces acting towards the points S and S' and in the proportion of r and q, or $\frac{r}{m}$ and $\frac{q}{m}$. How will this addition affect the locus of P? It should seem, for a reason similar to that before given, that the curve would remain the same; for the two new forces $\frac{r}{m}$ and $\frac{q}{m}$, acting in r or q or PS and PS' respectively, their resultant must, if there were none other acting, pass through the middle point C, between S and S'; and as we know that a force acting from that point, and in proportion to the distance from that point, causes the body to move in an ellipse whose centre is that point, and $r + q$ being constant, the ellipse must have the same axis and coincide with the ellipse produced by the combination of the forces $\frac{m}{r^2}$ and $\frac{m}{q^2}$.

7. This had appeared to be a necessary consequence of the conditions stated, but not as at all proving the velocity to be the same in the ellipse, when described by

* The condition of Legendre (mentioned in page 193), that $I^2 = V^2 + v^2$ is supposed to hold; for otherwise the centrifugal force would not be sufficient to balance the centripetal.

one force $\frac{m}{r^2}$ or $\frac{m}{q^2}$, or when described by the combined action of both, or when described by the combined action of $\frac{r}{m}$ and $\frac{q}{m}$, or of $\frac{m}{r^2}$, $\frac{m}{q^2}$, $\frac{r}{m}$, and $\frac{q}{m}$; because in all those cases the velocity will be different, and particularly the action of $\frac{m}{r^2} + \frac{r}{m}$ with $\frac{m}{q^2} + \frac{q}{m}$ will occasion a different velocity in each point from that occasioned by $\frac{m}{r^2} + \frac{m}{q^2}$. Thus to take the velocity at one point answering to C. If Πa and $\Pi a'$ be taken as $\frac{m}{r^2}$ and $\frac{m}{q^2}$, the diagonal $\Pi c'$ is the force of $\frac{m}{r^2}$ and $\frac{m}{q^2}$ combined, ΠC is the resultant of $\frac{r}{m}$ and $\frac{q}{m}$ combined (supposing $m = 1$). Therefore the velocity in Π will be as $\Pi c' + \Pi C$, when all the forces act, and only as $\Pi c'$ when the two former act alone, and as ΠC when the two latter act alone.* But the curves appear to be the same in each case.

8. These consequences seeming to follow from a consideration of the conditions stated, but without a full and rigorous investigation, it was very satisfactory to find that Lagrange had arrived at the same conclusion in one case of his solution of the problem of two fixed centres (*Mec. Anal.* part ii. sect. 7, chap. 3). That solution is marked throught with the stamp of his great genius. Euler had, in the *Berlin Memoirs* for 1760, treated the case of the inverse square of the distance and the centres and orbit being in the same plane. Lagrange's solution is general for

* The difference in velocity is easily obtained, in comparing the effect of one force and of the combined forces, from the equation $v^2 = \Sigma(f \times \text{half chord osculating circle, the chord being} = \frac{2p \cdot r}{r}$, p = perpendicular to the tangent, and R = radius of curvature.

the force being as any function of the distance, and of x, y, z, being the co-ordinates. Pressed by the great difficulties of the problem, and the impossibility of a general solution, he first confines himself to the inverse square of the distance (p. 97), and a general integration being still impossible, even after obtaining a differential equation with the variables separated, he makes a supposition which enables him to obtain two particular integrals (p. 99), and this gives for the orbit an ellipse in the one case and an hyperbola in the other, with the foci in the two centres of force; and it follows, he observes, from the investigation, that the same conic section which is described in virtue of a force to one focus, acting inversely as the square of the distance, or to the centre and acting in the direct ratio of the distance, may be still described in virtue of three such forces ("trois forces pareilles"*), tending to two foci and to the "centre." He adds: "Ce qui est très remarquable" (p. 101). It having appeared to many persons that a portion of the demonstration was not so rigorous as might be desired, M. Serret has very ably and satisfactorily supplied the defect (*Mec. An.* tom. ii. note iii. p. 329, ed. 1855), but he arrives at the same result. There is also given a very important generalization of Lagrange's solution, and of Legendre's theorem already mentioned, by M. Ossian Bonnet (*Ibid.* note iv.).

9. The same reason already given proves that if, instead of two points not in the trajectory we take two in it, as Σ and Σ', and refer the forces to those two, and make the forces $\frac{m}{r^5}$ and $\frac{m}{q^5}$ in $\Sigma\Pi'$ and $\Sigma'\Pi'$ respectively, and the angle of projection and initial force the same, the same circle will be described by the body; and that if two other forces

* It is plain that "pareilles" does not mean of the same kind as $\frac{1}{q^2}$ and ν; for he resolves the force to the centre into two acting to the foci, and calls the whole forces $\frac{\alpha}{r^2} + 2\gamma r$ and $\frac{\beta}{q^2} + 2\gamma q$.

also act on it, as $\Sigma\,\Pi'$ and $\Sigma'\,\Pi'$ $\left(\text{or } \frac{r}{m} \text{ and } \frac{q}{m}\right)$ the same circle will be described by the joint action of the forces. This is even a more remarkable consequence than the other; because the forces acting to the centre would of course give a uniform motion, and those acting to the points in the circumference an accelerated motion, and the forces combined will give an accelerated motion. At the middle point Π, the velocity will be, if only the forces $\frac{m}{r^5}$ and $\frac{m}{q^5}$ act, as $\frac{\sqrt{m}}{2a^2}$; if the forces $\frac{r}{m}$ and $\frac{q}{m}$ also act, it will be as $\sqrt{\frac{m}{4a^4} + \frac{2a}{m}}$. It must, however, be added, that Lagrange's solution does not contain this case of the circle and two points in the circumference, and there is very great difficulty in applying to it his analysis. Indeed, it appears that if the problem be worked upon the datum of $R = \frac{\alpha}{r^5} + 2\gamma r$, and $Q = \frac{\beta}{q^5} + 2\gamma q$, there is no possibility of obtaining an expression freed from the integral sign $\left(\int\right)$ in the same way as Lagrange does from his equation, founded upon the datum $R = \frac{\alpha}{r^2} + 2\gamma r$ and $Q = \frac{\beta}{q^2} + 2\gamma q$; $m = -2$, and consequently $m + 2 = 0$ seems necessary to his process.

There seems reason to suppose that the kind of reasoning on which we have relied as to the identity of the trajectories had influenced Legendre in confining his investigation to the case of curves which have not infinite branches. He expressly says (*Ex. de Calc. Int.* 11, 372), that he confines himself to curves where the orbit is restricted to a definite space. Certain it is, that the reasons applied to the identity in the case of curves returning into themselves is wholly inapplicable to curves having infinite branches.

10. The extreme complication of the problem arising from the resultants passing through innumerable points in the axis has been above noted, as regards the case of two forces only $\frac{m}{r^2}$ and $\frac{m}{q^2}$. When we add the other two $\frac{r}{m}$ and $\frac{q}{m}$ the complication is not considered by Lagrange to be increased (p. 99), and probably it is not as regards the analytical investigation. But it certainly is increased as regards the geometrical construction; for we then have to take the resultant of P c with P C (which is the resultant of r and q), and this will carry the ultimate diagonal representing the whole force applied to P beyond the axis S S'. Lagrange indeed does not take P C into his analysis, because he supposes the forces r and q to act in the same line of the radii vectores with the forces $\frac{m}{r^2}$ and $\frac{m}{q^2}$. But this would cause these radii vectores to be produced, and make their resultant also fall below the axis. It can hardly be doubted that these considerations weighed with Sir Isaac Newton, in disinclining him to the investigation of a problem which could afford no hope of a geometrical, or of any synthetical solution. That he had deeply considered the subject of attraction to various centres, in the more difficult case of moveable centres is certain. The justly celebrated LXVIth proposition of the First Book affords ample proof of it; and indeed the LXIVth proposition comes so near the subject of this note, that it may be correctly said to contain the grounds both of Clairaut's and Legendre's more full investigation.

11. In connection with this subject Lagrange expresses great admiration of a theorem of Lambert, which no doubt is remarkable, that in ellipses $\left(\text{the central force being as } \frac{1}{r^2}\right)$ the time taken to describe any arc depends only on the transverse axis, the chord of the arc, and the sum of the radii vectores at its extremities. We may observe, in passing, that

the vanishing of the expression for the conjugate axis in some fundamental formulæ connected with the ellipse, for example, the subtangent, gives rise to other curious properties of the curve similar to the one noted in this theorem, which is itself related to that peculiarity. (See a porism arising from this circumstance in Tract IV.) The same theorem had occurred to Lagrange himself, in examining the problem of deflecting forces to two centres; it is indeed derivable immediately from the case of that problem when one force vanishes and the centre connected with it is in an arc of the ellipse; for then the radius vector belonging to that centre becomes the chord. But Euler, long before either of them, in 1744, had given the theorem for parabolic arcs, which they only extended to elliptic arcs, and had published it in the *Berlin Mem.* 1760. Yet when Lambert claimed it as his own in 1771, and Lagrange gave him the honour of it in 1780, Euler, though he lived three years after, never thought of reminding them of his prior claims. It was thus, too, with the first of analysts, respecting the extension of the Differential Calculus to that of Partial Differences (Tract III.), by far the greatest step in mathematical science which has been made since the age of Newton and Leibnitz, if it have not a rival in the calculus of variations, the honour of which also is shared by him with Lagrange.

12. It must be observed that when in 1771 (*Berlin Mem.*) Lambert extended the theorem to elliptic arcs, he was ignorant of Euler having anticipated him as to parabolic arcs. But Lagrange truly states (*Mec. Anal.* ii. 28, ed. 1855), what shows that all of them had been anticipated by Newton. For in the IV. and V. Lemmas of the Third Book he had very distinctly given the whole materials of the proposition as far as parabolic arcs are concerned.

Lagrange notes the uses of the theorem, and observes upon the remarkable circumstance of the time not depending at all on the form of the ellipse, providing the transverse axis remains the same. This must have frequently recurred to his recollection, when engaged in those great investigations

which show the connection that the transverse axis remaining unchanged has with the permanency of the system.

13. He further remarks upon another consequence of the conjugate axis, or the form of the orbit, not affecting the time; namely, that the conjugate wholly disappearing, and the orbit becoming rectilinear, the theorem applies to the time of falling to the centre, on the centrifugal force or that of projection ceasing to act. (*Berlin Mem.* 1778.) But Newton's VIth Lemma, to which he does not refer, in some degree anticipated this also.

14. The great difficulty of the problem of several centres has been stated. Euler was clearly of this opinion, and he was the first that undertook the solution. After speaking of the general problem (*Berlin Mem.* 1760, p. 228) as alike important and difficult, he confines himself to the case of two bodies in fixed positions, acting upon a third, which moves in the plane of those disturbing bodies; in a word, to the motion of a body drawn towards two fixed centres. He says that, whoever undertakes the solution of this less difficult problem "will find difficulties almost as insurmountable as in the great fundamental problem of astronomy;" and adds that, after making many fruitless attempts, he had at last been led to a solution by the accident of an error into which he had fallen in his investigation. What he proposes is to find the cases in which the curve is algebraical; there being, according to the conditions, an infinite variety, most of them transcendental. He considers, however, that if this case of two bodies in fixed centres, and in the same plane with the body attracted, should be incapable of solution, the general problem must prove still more so. Nothing can exceed the clearness of his investigation; and the ingenious subtlety of the contrivances by which he facilitates the reduction of his differential equations to those of a lower degree. Of this Lagrange expresses great admiration, who, in giving a solution of the case in some respects more extended, but in others less, became fully sensible of the difficulties of the process,

and whose investigation is less luminous than his great predecessor's. Euler reduces his investigation to the integration of the equation

$$\frac{\mu\, dx}{\sqrt{x + x^3}} = \frac{\nu\, dy}{\sqrt{y + y^3}};$$

and obtaining the relation between the angles made by the two radii vectores with the axis. It is clear that Lagrange's solution is obtained by another course altogether.

FORCE VARYING INVERSELY AS THE DISTANCE.

It is remarkable that what at first sight seems to be the most simple of all the cases, that of the central force varying inversely as the distance, or of $m = 1$ in $\frac{\mu}{r^m}$, should be found so much the most difficult of solution, and that, whether the proportion of $\frac{1}{r}$ enters into motion related to one centre only or to more centres than one. Herman, in the 'Phoronomia,' turns away from it, merely observing that his formula fails when $m = 1$. Clairaut, in his excellent commentaries on the 'Principia,' his additions to Madame du Chatelet's translation, deduces, chiefly from the Propositions of the Second and Eighth Sections (lib. i.), a general differential equation for the curve described by a body under the influence of a centripetal force as Y, a function of the radius vector; and the equation is therefore a polar one. It involves the integration of $\int \mathrm{Y}\, dy$. Consequently, when $\mathrm{Y} = \frac{1}{y}$, the case we are now considering, the integral contains an unmanageable logarithm; for the equation becomes

$$b^2 f^2\, d\, x = \frac{dy}{y\left(2\,\mathrm{B} - 2\int \frac{dy}{y}\right)^{\frac{1}{2}}} = \frac{dy}{y^2\,(2\,\mathrm{B} - \log y^2)^{\frac{1}{2}}}.$$

He makes no mention of this case, as, like Herman and most

others, he seems unwilling to approach it; undoubtedly, however, such is the application of his formula.

Keil, in his paper on Central Forces in 'Phil. Trans.' 1708, p. 174, gives the case of the force as $\frac{1}{r}$ and reduces it to finding $\frac{d}{(b - \log r)^{\frac{1}{2}}} = \mathrm{P}$, the perpendicular to the tangent.

By one process grounded on Prop. XLI., Lib. I., this result is obtained for the case of $\frac{1}{r}$, that is $\frac{1}{(x^2 + y^2)^{\frac{1}{2}}}$ $\int\int \frac{2\,dx\,d^2x + 2\,dy\,d^2y}{dt^2} = \int 2\mu \frac{(x\,dx + y\,dy)^{\frac{1}{2}}}{x^2 + y^2}$, or $\int \frac{dx^2 + dy^2}{dt^2} - 2\log(x^2 + y^2) - c = 0$; and dt^2 being $= \frac{(y\,dx - x\,dy)^2}{c^2}$, the equation becomes $\int \frac{dx^2 + dy^2}{(y\,dx - x\,dy)^2}$ $- \frac{\mu}{c^2}\log(x^2 + y^2) + \frac{v}{c^2} = 0$.

The process grounded on the formula $f = \frac{hr}{2p^3 \cdot \mathrm{R}}$ is, if possible, more hopeless; for this gives

$$\frac{h(y^2 + (x - c)^2)^{\frac{1}{2}} \times (dx\,d^2y - dy\,d^2x)}{2(y\,dx - x\,dy + c\,dy)^3} = \frac{1}{(y^2 + (x - c)^2)^{\frac{1}{2}}},$$

or $h(y^2 + (x - c)^2)(dx\,d^2y - dy\,d^2x) = 2(y\,dx - x\,dy + c\,dy)^3$, or $\frac{h\,dy}{dx} = \int \frac{2(y\,dx - x\,dy + c\,dy)^3}{dx^2(y^2 + (x - c)^2)}$.

The difficulty follows $\frac{1}{r}$ wherever that proportion enters into the investigation. Thus in the problems connected with different centres, when it is found that forces varying as $\frac{1}{r^2}$ and $\frac{1}{q^2}$, being combined with forces varying as the dis-

tance directly, or as r and q, give an elliptic orbit, the resultant of the latter forces passes through the centre, and the locus of that resultant is the opposite semi-ellipse, and so of a circle. But when the proportion is $\frac{1}{r}$ and $\frac{1}{q}$, (also if the force towards each centre is as the radius vector to the other centre), the resultant passes through innumerable points to an opposite curve, sometimes of a different kind, although each resultant differing in its direction from all the others, and in the case of the circle, from the diameter, is equal to the one passing through the middle point of the line joining the two centres. In this case, therefore, there is no combined action of the forces $\frac{1}{r^2}$ and $\frac{1}{q^2}$, or $\frac{1}{r^5}$ and $\frac{1}{q^5}$ or of their several resultants, with the resultant of $\frac{1}{r}$ and $\frac{1}{q}$, as there is in the case of $\frac{r}{m}$ and $\frac{q}{m}$, but the several forces act wholly in the direction of the radii vectores severally.

It evidently appears to be a more simple and natural combination that the two sets of forces should diminish with the distance increasing, as in $\frac{1}{r^2}$ and $\frac{1}{q^2}$ combined with $\frac{1}{r}$ and $\frac{1}{q}$, than that one set should decrease and another increase with the distance, as in $\frac{1}{r^2}$ and $\frac{1}{q^2}$ with r and q, in which case there must even be an extinction of force at one point, where (taking the sum of the forces instead of their resultant) $\frac{m}{r^2} + \frac{m}{q^2} = \frac{r+q}{m}$, or r is as in the equation $r^3 + \frac{q^3 - m^2}{q^2} r^2 = m^2$. Of course the value of q would be the same; and the resultant (more accurately taken to measure the increase of the force) would at one value give the two sets of forces as counterbalanced.

The younger Euler (J. A. Euler) has a paper in the *Berlin Mem.* 1760, p. 250, upon the action of a central force decreasing as the distance, in the case of the attracted body's descent towards the centre, and states the reason of this problem being insoluble except by arcs or logarithms. He finds that taking a = the height from which the descent begins, f = that at which the centripetal force is equal to the gravity of the attracted body, the time of descent towards the centre is $= \frac{1}{\sqrt{f}} \int \frac{dy}{\sqrt{\log \frac{a}{y}}}$, y being the distance from the centre.*

* This Tract is from Appendix III. and IV. to the Analytical View of the Principia, pp. 424, 421.

X.

METEORIC STONES.

The histories of all nations, in early times, abound with fabulous accounts of natural phenomena. Showers of blood and of flesh; battles of armed men in the air; animals of different descriptions uttering articulate sounds—are a few of the tales which we meet with in the annals of ancient Rome: and the lively imagination of Oriental countries has infinitely varied this catalogue of wonders. Of such incidents, however, it has frequently been found possible to give some explanation consistent with the ordinary laws of nature, after the narratives have been freed from the fictions with which superstition or design had at first mingled them. But it is singular with what uniformity the notion of showers of stones has prevailed in various countries, at almost every period of society; with how few additions from fancy the story has been propagated; and how vain all attempts have proved, to account, by natural causes, for the phenomenon, with whatever modifications it may be credited. Accordingly, philosophers have rejected the fact, and either denied that stones did fall, or affirmed, at least, that if they fell on one part of the earth, they were previously elevated from another. The vulgar have as stedfastly believed that they came from beyond the planet on which we live; and every day's experience seems now to increase the probability, that in this instance, as in some others, credulity has been more philosophical than scepticism.

There are two methods of inquiring into the origin of those insulated masses which are said to have fallen into different

parts of the earth. We may either collect, as accurately as possible, the external evidence, the testimonies of those persons in whose neighbourhood the bodies are situated; or we may examine the nature of the substances themselves, and compare them with the kinds of matter by which they are surrounded. The first mode of investigation is evidently more liable to error, and less likely to proceed upon full and satisfactory *data* than the other. But if both inquiries lead to conclusions somewhat analogous; if both the inductions of fact present us with anomalous phenomena of nearly the same description, and equally irreducible to any of the classes into which all other facts have been arranged, we may rest assured that a discovery has been made—and the two methods of demonstration will be reciprocally confirmed.

I. The first narrative which has been offered to the world, under circumstances of tolerable accuracy, is that of the celebrated Gassendi. He was himself the eyewitness of what he relates. On the 27th of November, in the year 1627, the sky being quite clear, he saw a burning stone fall on mount Vaisir, between the towns of Guillaumes and Perne in Provence. It appeared to be about four feet in diameter, was surrounded by a luminous circle of colours like a rainbow, and its fall was accompanied with a noise like the discharge of cannon. But Gassendi inspected the supposed fallen stone still more nearly; he found that it weighed 59 lib., was extremely hard, of a dull metallic colour, and of a specific gravity considerably greater than that of common marble. Having only this solitary instance to examine, he concluded, not unnaturally, that the mass came from some neighbouring mountain, which had been in a transient state of volcanic eruption.

The celebrated stone of Ensisheim is not proved to have fallen by testimony quite so satisfactory; but there are several circumstances narrated with respect to it, which the foregoing account of Gassendi wants. Contemporary writers all agree in stating the general belief of the neighbourhood, that on the 7th of November 1492, between eleven and

twelve o'clock, A.M., a dreadful thunder-clap was heard at Ensisheim, and that a child saw a huge stone fall on a field sowed with wheat. It had entered the earth to the depth of three feet; it was then removed, found to weigh 260 lib., and exposed to public view. The defect in Gassendi's relation is here supplied; for we have the nature of the ground distinctly described: the natives of the place must have known that in their wheat-field no such stone had formerly existed: but the evidence of its having actually been observed to fall is by no means so decisive as that of Gassendi.

Other recitals have been given of similar appearances, but by no means so well authenticated, or so fully examined, although somewhat nearer our own times. In 1672, one of the members of the Abbé Bourdelot's academy presented at one of the meetings, a specimen of two stones which had lately fallen near Verona; the one weighed 300, the other 200 lib. The phenomenon, he stated, had been seen by three or four hundred persons. The stones fell in a sloping direction during the night, and in calm weather. They appeared to burn, fell with a great noise, and ploughed up the ground. They were afterwards taken from thence, and sent to Verona. This account, it may be observed, was published in the same year. Paul Lucas the traveller relates, that when he was at Larissa, in 1706, a stone of 72 lib. weight fell in the neighbourhood. It was observed, he says, to come from the north, with a loud hissing noise, and seemed to be enveloped in a small cloud, which exploded when the stone fell. It smelt of sulphur, and looked like iron dross.

M. De la Lande, in 1756, published an account of a phenomenon very nearly resembling the above, but deficient in several points of direct evidence. His narrative, however, deserves our attention, because he seems to have been upon the spot, and to have examined, with great care, the truth of the circumstances which he describes. In September 1753, during an extremely clear and hot day, a noise was heard in the neighbourhood of Pont-de-Vesle, resembling the discharge of artillery. It was so loud as to reach several leagues in all

directions. At Liponas, three leagues from Pont-de-Vesle, a hissing sound was remarked; and at this place, as well as at Pont-de-Vesle, a blackish mass was found to have fallen in ploughed ground, with such a force as to penetrate half a foot into the soil. The largest of these bodies weighed 20 lib.; and they both alike appeared, on the surface, as if they had been exposed to a violent degree of heat. It may here be observed, that the small depth at which these bodies were found in the ploughed land, renders it in the highest degree improbable that they should have existed there previously to the time of the explosion. To the same purpose we may remark the complete resemblance of the two masses found at so great a distance from each other.

In the year 1768, no less than three stones were presented to the Academy of Sciences at Paris, all of which were said to have fallen in different parts of France; one in the Maine, another in Artois, and the third in the Cotentin. These were all externally of the very same appearance; and Messrs. Fougeraux, Cadet, and Lavoisier drew up a particular report upon the first of them. They state, that on the 18th of September 1768, between four and five o'clock in the evening, there was seen near the village of Lucè, a cloud in which a short explosion took place, followed by a hissing noise, without any flame; that some persons about three leagues from Lucè, heard the same sound, and, looking upwards, perceived an opaque body which was describing a curve line in the air, and was about to fall upon a piece of green turf in the neighbouring high road; that they immediately ran to this place, and found a kind of stone, half buried in the earth, extremely hot, and about $7\frac{1}{2}$ lib. weight. This account of the fact was communicated to the academicians by the Abbé Bachelay. But they do not appear to have attached much credit to the whole circumstances of his narrative; for they conclude (chiefly from several experiments made to analyze it) that the stone did not fall upon the earth, but was there before the thunder-clap, and was only heated and exposed to view by the stroke of the electric fluid.

Of late years, the attention of philosophers has been more anxiously directed to this curious subject; and more accurate accounts of the supposed fall of stones have been collected from various quarters. It is not a little singular that the narrative which, of all others, was supported by the very best and most direct evidence, was treated by naturalists near the spot, with perverse incredulity, until the results of chemical analysis, about ten years after the thing happened, began to operate some change upon the common opinions relating to such matters. We allude to the shower of stones which fell near Agen, 24th July 1790, between nine and ten o'clock at night. First, a bright ball of fire was seen traversing the atmosphere with great rapidity, and leaving behind it a train of light which lasted about fifty seconds; a loud explosion was then heard, accompanied with sparks which flew off in all directions. This was followed, after a short interval, by a fall of stones, over a considerable extent of ground, at various distances from each other, and of different sizes; the greater number weighing about half a quarter of a pound, but many a vast deal more. Some fell with a hissing noise, and entered the ground: others (probably the smaller ones) fell without any sound, and remained on the surface. In appearance, they were all alike. The shower did no considerable damage; but it broke the tiles of some houses. All this was attested in a *procès-verbal*, signed by the magistrates of the municipality. It was further substantiated by the testimony of above three hundred persons, inhabitants of the district; and various men, of more than ordinary information, gave the very same account to their scientific correspondents. One of these (M. D'Arcet, son of the celebrated chemist of that name) mentions two additional circumstances, of great importance. from his own observation. The stones, when they fell upon the houses, had not the sound of hard and compact substances, but of matter in a soft, half-melted state; and such of them as fell upon straws adhered to them, so as not to be easily separated. It is utterly impossible to reconcile these facts with any other

supposition, than that of the stones having fallen from the air, and in a state of fusion. That they broke the roofs of houses, and were found above pieces of straw adhering to them, is the clearest of all proofs of their having fallen from above.

Although nothing can be more pointed and specific than this evidence, it yet derives great confirmation from the similar accounts which have still more recently been communicated. On the 18th December 1795, the weather being cloudy, several persons in the neighbourhood of Captain Topham's house, in Yorkshire, heard a loud noise in the air, followed by a hissing sound, and afterwards felt a shock, as if a heavy body had fallen to the ground at a little distance from them. One of these, a ploughman, saw a huge stone falling towards the earth, eight or nine yards from the place where he stood. It was seven or eight yards from the ground when he first observed it. It threw up the mould on every side, and buried itself twenty-one inches. This man, assisted by others who were near the spot at the time, immediately raised the stone, and found that it weighed about 56 lib. These statements have been authenticated by the signatures of the people who made them.

On the 17th March 1798, a body, burning very brightly, passed over the vicinity of Ville Franche, on the Saone, accompanied with a hissing noise and leaving a luminous track behind it. It exploded with great noise, about twelve hundred feet from the ground; and one of the shivers, still luminous, being observed to fall in a neighbouring vineyard, was traced. At that spot, a stone above a foot in diameter, was found to have penetrated about twenty inches into the soil. It was sent to M. Sage, of the National Institute, accompanied by a narrative of the foregoing circumstances, under the hand of an intelligent eyewitness.

While these observations in Europe were daily confirming the original but long-exploded idea of the vulgar, that many of the luminous meteors observed in our horizon are masses of ignited matter, an account of a phenomenon, precisely of the

same description, was received from the East Indies, vouched by authority peculiarly well adapted to secure general respect. Mr. Williams, a member of the Royal Society of London, residing in Bengal, having heard of an explosion, accompanied by a descent of stones, in the province of Bahar, made all possible inquiries into the circumstances of the phenomenon, among the Europeans who happened to be on the spot. He learnt, that on the 19th December 1798, at eight o'clock P.M., a luminous meteor, like a large ball of fire, was seen at Benares, and in different parts of the country; that it was attended with a rumbling, loud noise; and that, about the same time, the inhabitants of Krakhut, fourteen miles from Benares, saw the light, heard a loud thunder-clap, and, immediately after, heard the noise of heavy bodies falling in their neighbourhood. Next morning the fields were found to have been turned up in different spots, which was easily perceived, as the crop was not more than two or three inches above the ground: and stones of different sizes, but apparently of the same substances, were picked out of the moist soil, generally from a depth of six inches. As the occurrence took place in the night, and after the people had retired to rest, no one observed the meteor explode, or the stones fall; but the watchman of an English gentleman who lived near Krakhut, brought him one next morning, which he said had fallen through the top of his hut, and buried itself in the earthen floor.

Several of the foregoing narratives mention the material circumstance, of damage done to interposed objects by the stones supposed to have fallen on the earth. In one instance, still more distinct traces were left of their progress through the air. During the explosion of a meteor, on the 20th August 1789, near Bordeaux, a stone, about fifteen inches diameter, broke through the roof of a cottage, and killed a herdsman and some cattle. Part of the stone is now in the museum of Mr. Greville, and the rest in that of Bordeaux. It is singular that this fact is not mentioned by M. Izarn, in his work on the subject of these stones, nor by Vauquelin,

although he examined a specimen evidently taken from the same stone, and received a *procès-verbal* of the manner in which it fell. We take the account from Mr. Greville's paper (Phil. Trans. 1803, part I.); and he appears to have received it from M. St. Amand, Professor of Natural History at the Central School of Agen.

It is quite impossible, we apprehend, to deny very great weight to all these testimonies; some of them given by intelligent eyewitnesses; others by people of less information, indeed, but prepossessed with no theory; all concurring in their descriptions, and examined by various persons of acuteness and respectability, immediately after the phenomena had been exhibited. Without offering any further remarks, then, upon this mass of external evidence, we shall only remind the reader of the main points which it seems satisfactorily to substantiate. It proves, that, in various parts of the world, luminous meteors have been seen moving through the air, in a direction more or less oblique, accompanied by a noise, generally like the hissing of large shot, followed by explosion, and the fall of hard, stony, or semi-metallic masses, in a heated state. The hissing sound, so universally mentioned; the fact of stones being found, unlike those in the neighbourhood, at the spots towards which the luminous body or its fragments were seen to move; the scattering or ploughing up of the soil at those spots, always in proportion to the size of the stones; the concussion of the neighbouring ground at the time; and, above all, the impinging of the stones upon bodies somewhat removed from the earth, or lying loose upon its surface—are circumstances perfectly well authenticated in these reports; and, when taken together, are obviously fatal to any theory, either of the masses having previously existed in the soil ready formed, and having been disclosed by the the electric fluid—or of their component parts having existed there, and having been united and consolidated by that fluid.

II. While the internal evidence on this question, that is, the inference arising from an examination of the stones them-

selves, agrees most harmoniously with the conclusion to which the narratives above analyzed force our assent, and greatly strengthens that conclusion, it also leads to a further knowledge of the subject, than the mere external evidence could of itself have afforded us.

The reports from all those who observed the meteors, and found the stones in the neighbourhood, after the explosions, agree in describing those substances as different from all the surrounding bodies, and as presenting, in every case, the same external appearance of semi-metallic matter, coated on the outside with a thin black crust, and bearing strong marks of recent fusion. This general resemblance we should be perfectly entitled to infer from the various accounts of eye-witnesses, even if no more particular observations had been made by men of science, to whose inspection many of the fallen bodies were submitted. But fortunately a considerable number of these singular substances have been examined, with the greatest care, by the first chemists and naturalists of the age; and their investigations have put us in possession of a mass of information, capable of convincing the most scrupulous inquirer that the bodies in question have a common origin, and that we are as yet wholly unacquainted with any natural process which could have formed them on our globe.

M. De la Lande appears to have examined the stones which fell near Bourg, in the province of Bresse, 1753, with some attention He remarks their external coating of black vitrified matter, the metallic or pyritical threads interspersed through them, and more particularly the cracks filled with metallic particles. His chemical analysis is very meagre and unsatisfactory; but such as it was, its results, as well as the general observations of external character, corresponded with the inferences drawn by him from a similar examination of the stone which fell in 1750, near Coutances, in Normandy, at the distance of three hundred and sixty miles from Bourg.

The external appearance of the three stones presented to the Academy of Sciences, as having fallen in different parts of

France during the year 1768, was precisely the same. But Messrs. Lavoisier, &c., the committee appointed to examine them, performed the chemical analysis with much greater accuracy and fulness than M. De la Lande, who was no chemist, had done. That which fell in the Maine, and was presented by the Abbé Bachelay, underwent the most careful process. It was found to contain, of sulphur, $8\frac{1}{2}$ *per cent.*; iron, 36; and vitrifiable earth, $55\frac{1}{2}$. It must be remarked, however, that this decomposition was effected by means of experiments performed upon an integral part of the whole stone, considered as a homogeneous substance; whereas, it is in fact a congeries of substances which ought to have been separately analyzed. This consideration will, in part at least, enable us to account for the apparent discrepancy between the results obtained by the academicians and those of later experimentalists. Messrs. Lavoisier, &c., also examined particularly another stone, said to have fallen in a different part of France, and obtained very nearly the same results. The only difference was, that it did not give out sulphurated hydrogenous gas when acted upon by the muriatic acid; a peculiarity distinctly observable in the other substance.

The description which Professor Barthold gives of the external character of the stone which fell near Ensisheim, in the fifteenth century, corresponds exactly with the descriptions given of these stones, and of the ores examined by M. De la Lande. The results of this analysis are somewhat different; but he examined the whole heterogeneous compound, and not the parts separately. He concluded, that this mass contained 2 *per cent.* of sulphur, 20 of iron, 14 magnesia, 17 alumina, 2 lime, 42 silica. Mr. Howard has very justly remarked, that the Professor's own account of his experiments is at variance with the idea of lime being contained in the substance; and that he has given no sufficient proof of the existence of alumina. It is also to be observed, that from the exceptionable method of analysis pursued both by Barthold and the academicians, the metallic particles were not examined with sufficient precision. The specific gravity of the

stones examined by the academicians was to that of water, as 3535 to 1000. The specific gravity of the stone of Ensisheim, as tried by Barthold, was 3233; that of the stone examined by Gassendi (who saw it fall) was 14, common marble being 11; and, taking the specific gravity of marble to that of water, as 2716 to 1000, the specific gravity of the stone observed by Gassendi will be to that of water as 3456 to 1000. So near a coincidence between observations, made at such a distance of time, upon these various substances, cannot fail to strike us as very remarkable, and to prepare us for that fuller demonstration of their identity, which was reserved for the labours of our countryman, Mr. Howard.

This excellent philosopher has elucidated the subject of our present consideration, by a course of experiments as interesting and instructive as any that the science of chemical analysis can boast of. He fortunately obtained specimens of the stones which fell in several very distant quarters of the globe; at Benares, and in Yorkshire (as we have already described); near Sienna, and in Bohemia, according to evidence not altogether so satisfactory as that upon which the other narratives rest.

He began his inquiries, very judiciously, by a minute examination of the external mineralogical characters of these four substances; and in this part of his task he was indebted to the learning and expertness of the Count de Bournon. The substances were found to resemble each other very closely in their general appearance, and in the nature of their component parts. The chief difference consisted in the different proportions in which the same component parts were combined, so as to form the aggregate of the heterogeneous masses. Their specific gravities were nearly the same, unless that the abundance of iron in one of the masses caused a considerable increase of its gravity. It may contribute to the formation of a precise estimate, if we present, in one view, the results of the experiments made to measure the specific gravities of the most remarkable specimens hitherto examined. The four last in the list were calculated

by the Count de Bournon. The specific gravity of water being 1000,

That of the	Ensisheim stone	is	3233
,,	Gassendi's *	,, ...	3456
,,	Bachelay's †	,, ...	3535
,,	Yorkshire	,, ...	3508
,,	Sienna	,, ...	3418
,,	Benares	,, ...	3352
,,	Bohemia	,, ...	4281

All the stones examined by Count de Bournon and Mr. Howard were found to consist of four distinct substances; small metallic particles; a peculiar martial pyrites; a number of globular and elliptical bodies, also of a peculiar nature; and an earthy cement surrounding the other constituent parts. It was only the stone from Benares that Mr. Howard could separate into its constituent parts, with sufficient accuracy, and in sufficient abundance, for a minute analysis of each. He found, however, that the nature of the metallic particles was the same in all; they were in each case an alloy of iron and nickel. In the pyrites of the Benares stone, nickel as well as iron was detected; and the easy decomposition of the pyrites by muriatic acid, in all the specimens, afforded a distinguishing character of this substance. The globules in the Benares stone contained silica, magnesia, and oxides of nickel and iron; the earthy cement consisted of the same substances, very nearly in the same proportions. In the other stones, these globules could not be easily separated from the cement and pyrites. Mr. Howard, therefore, after freeing the aggregate as well as possible from the metallic particles, and several of the globules, was obliged to satisfy himself with analyzing the heterogeneous mass. Still the composition appeared wonderfully to agree with that of the basis and globules of the Benares stone; as the following Table, which we have collected from Mr. Howard's experiments, and reduced to the parts of a hundred, will clearly evince.

* Found in Provence. † Found in the Maine.

	Oxide of Nickel.	Oxide of Iron.	Magnesia.	Silica.
Stone from Benares . {Globules . .	2·5	34·	15·	50·
{Cement . . .	2·5	34·	18·	48·
Stone from Yorkshire. Basis, *i. e.* earthy cement, with some globules and the pyrites, deprived of its sulphur . .	1·3	32·	24·6	50·
Stone from Sienna. Basis	2·	34·6	22·6	46·6
Stone from Bohemia. Basis	2·7	42·7	17·2	45·4

About the time that Mr. Howard was engaged in these interesting researches, and before he had published the result of them, M. Vauquelin happened also to be occupied with the very same subject. He analyzed, though by a different process, the Benares stone, and two others which fell in 1789 and 1790 in the south of France. The results of his experiments agreed with those of our distinguished countryman in every particular; and we are now entitled to conclude, with perfect confidence, that the stones which have at different times fallen upon the earth, in England, France, Italy, and the East Indies, are precisely of the same nature, consisting of the same simple substances arranged in similar compounds, nearly in the same proportions, and combined in the same manner, so as to form heterogeneous aggregates whose general resemblance to each other is complete. We are further warranted in another important inference, that no other bodies have as yet been discovered on our globe which contain the same ingredients; and, more particularly, that the analysis of these stones has made us acquainted with a species of pyrites not formerly known, nor anywhere else to be found.

The general analogy between these stones and the masses of native iron found in different parts of the world, was too striking to escape the eminent inquirers who have investigated this subject. They resemble each other in their external character, though not by any means so closely as the stones; but in one circumstance of their chemical composition they have a remarkable similarity, both among themselves,

and towards the stony substances. M. Proust, a considerable time before the date of Mr. Howard's discoveries, had proved that the enormous mass of native iron found in South America, contained a large portion of nickel in its composition. Mr. Howard was led to the same conclusion by analyzing another portion of this body; and he found that the solitary masses discovered in Siberia, Bohemia, and Senegal, contained a mixture of the same metal with iron, though in various proportions. The Bohemian iron is an alloy, of which nickel forms eighteen parts in the hundred; in the Siberian iron, it forms seventeen: and in the Senegal iron, five or six. But what is still more striking, and tends to place the similarity of their origin beyond all doubt, the Siberian mass is interspersed with cavities, containing an earthy substance of the very same nature as the earthy cement and globules of the Benares stone; nay, the proportions of the ingredients, according to Mr. Howard's analysis, are nearly alike, if we except that of the oxide of iron, which is considerably smaller in the Siberian earth. This curious fact excites the strongest prepossession in favour of the idea, that the Siberian iron owes its origin to the same causes which formed and projected the different stones supposed to have fallen on the earth: and, coupled with the other details of the analysis, it naturally leads us to conclude, that the masses of native iron, as they are called, differ in no respect from the metallic particles, or the alloy of iron and nickel, which constitute one of the four aggregate parts in every stone hitherto examined.

It may be remarked, that, excepting the tradition of the Tartars respecting the fall of the Siberian iron from heaven, no external evidence has been preserved to illustrate the origin of those masses of native metal which have been analyzed by chemists. A tolerably authentic testimony has, however, lately been found, to prove the fall of a similar body in the East Indies. Mr. Greville has communicated to the Royal Society (Phil. Trans. 1803, part I.), a very interesting document, translated from the Emperor Tchangire's

Memoirs of his own reign. The Prince relates, that in the year 1620 (of our era), a violent explosion was heard at a village in the Punjaub, and during the noise, a luminous body fell from above on the earth. That the amnil (or fiscal officer) of the district immediately repaired to the spot where the body was said to have fallen, and having found it to be still hot and not burnt up, caused it to be dug up; when the heat increasing, he at last came to a lump of iron violently hot; that this was sent to court, where the emperor had it weighed in his presence, and ordered it to be forged into a sabre, a knife, and a dagger; that the workmen reported that it was not malleable, but shivered under rhe stroke; and that it required to be mixed up with one-third part of common iron, when the mass was found to make excellent blades. The royal historian adds, that upon the incident of this *iron of lightning* being manufactured, a poet presented him with a distich, purporting that, "during his reign, the earth attained order and regularity; that raw iron fell from lightning, and was, by his world-subduing authority, converted into a dagger, a knife, and two sabres."

The exact resemblance of the occurrence here related, in all its essential circumstances, to the accounts of fallen stones formerly detailed, and the particular observation upon the unmalleable nature of the iron, give, it must be confessed, a very great degree of credibility to the whole narrative, and bestow additional weight on the inference previously drawn from internal evidence, that the solitary masses of native iron, found in different quarters of the globe, have the same origin with the stones analyzed by Vauquelin and Howard.

We have now gone through the whole evidence, both with respect to the circumstances in which these singular bodies are found, the ingredients of which they are compounded, and the outward appearance and structure which they exhibit: we are next to consider the inferences respecting their probable origin, which this mass of information may warrant us to draw.

Independent of the distinct negative which the external evidence gives to any such conclusions, we are fully entitled to deny that these bodies are formed in the ground by lightning, or existed previously there, both from their exact resemblance to each other in whatever part of the earth they have been found, and from their containing substances nowhere else to be met with. It cannot surely be imagined, that exactly in those spots where fire, of some unknown kind, precipitated from an exploded meteor, happened to fall, there should exist certain proportions of iron, sulphur, nickel, magnesia and silica, ready to be united by the heat or electricity. Still less conceivable is it, that in every such fall of fire, those ingredients should first combine, by twos and threes, in the very same manner, and then that the binary and ternary compounds should unite in similar aggregates. But, least of all is it reasonable to suppose, that bodies formed in the earth should, upon being dug up, be found enveloped in a crust different from the rest of their substance, and bearing evident marks of having undergone the action of heat in contact with the air.

The same unquestionable resemblance which prevails among all these bodies, and, still more, the peculiar nature of the pyrites which they contain, prove very clearly that they have not a volcanic origin. Even if such an hypothesis were liable to no other objection, it would be inadmissible on this ground, that we know of no volcano which throws up so small a portion of matter, and so uniformly of the same kind. But though we were to admit the existence of this volcano, where must we place it, that its eruptions may extend from Bengal to England, France, Italy, and Bohemia; nay, from Siberia to Senegal and South America? And if we are forced to admit the existence of a series of such volcanoes, which are known to us only by these peculiar effects of their eruptions, do we not acknowledge that we are compelled to imagine a set of causes, without any other foundation for our belief in them, than our occasion for their assistance in explaining the phenomenon? In short, do we not account for one difficulty,

by fancying a greater? But if it is alleged that the stones come from volcanoes already known, we demand, what volcano exists in the Peninsula of India, or in England, or in France, or in Bohemia? And if it is said that these bodies are projected by Hecla, Ætna, Vesuvius, to all manner of distances, we must ask, whether this is not explaining what is puzzling, by assuming what is impossible? It is surely much better to rest satisfied with recording the fact, and leaving it under all its difficulties, than to increase its wonders by the addition of a miracle.

The same remark may be extended to those who have fancied that the constituent parts of the stones exist in the atmosphere, and are united by the fire of a meteor, or by the electric fluid. We have no right to make any such hypothesis. We have never seen iron, silica, nickel, in the gaseous state. These bodies may, for aught we know, be compounds of oxygen and azote or hydrogen, &c.; but as yet we have no reason to think so. Besides, he who amuses us with this clumsy and gratuitous explication, will probably account for every other phenomenon by a similar process of creation. He may, with equal plausibility, conceive the earth to be formed by a union of burnt gases, and then cover it with vegetables, and people it with living creatures, by a few more conflagrations and explosions. Such, however, is the theory most heavily expounded by M. Izarn—spun, with tiresome and unprofitable industry, into cobwebs, which touch every fact, without catching it—and enveloped in the mist of general logical positions, which faintly conceal the fundamental postulate—an entire act of creation.

From the whole, we may safely infer, that the bodies in question have fallen on the surface of the earth, but that they were not projected by any volcanoes, and that we have no right, from the known laws of nature, to suppose that they were formed in the upper regions of the atmosphere. Such a negative conclusion seems all that we are, in the present state of our knowledge, entitled to draw. But an hypothesis may perhaps suggest itself, unencumbered

by any of the foregoing difficulties, if we attend to the following undoubted truths.

As the attraction of gravitation extends over the whole planetary system, a heavy body, placed at the surface of the moon, is affected chiefly by two forces; one drawing it towards the centre of the earth, and another drawing it towards that of the moon. The latter of these forces, however, is beyond all comparison greatest at or near the moon's surface. But as we recede from the moon, and approach to the earth, this force decreases, while the other augments; and at one point between the two planets these forces are exactly equal—so that a heavy body, placed there, must remain at rest. If, therefore, a body is projected from the moon towards the earth, with a force sufficient to carry it beyond this point of equal attraction, it must necessarily fall on the earth. Nor would it require a very great impulse to throw the body within the sphere of the earth's superior attraction. Supposing the line of projection to be that which joins the centres of the two planets, and supposing them to remain at rest; it has been demonstrated, on the Newtonian estimation of the moon's mass, that a force of projection moving the body 12,000 feet in a second, would entirely detach it from the moon, and throw it upon the earth. This estimate of the moon's mass is, however, now admitted to be much greater than the truth; and upon M. De la Place's calculation, it has been shown that a force of little more than one half the above would be sufficient to produce the effect. A projectile, then, moving from the moon with a velocity about three times greater than that of a cannon ball, would infallibly reach the earth; and there can be little doubt that such forces are exerted by volcanoes during eruptions, as well as by the production of steam, from subterranean heat. We may easily imagine such cause of motion to exist in the moon, as well as in the earth. Indeed, several observations have rendered the existence of volcanoes there extremely probable. In the calculation just now referred to, we may remark, that no allowance is made for the resistance of any medium in the

place where the motion is generated. In fact, we have every reason to believe, from optical considerations, that the moon has no atmosphere.

A body falling from the moon upon the earth, after being impelled by such force as we have been describing, would not reach us in less than two days and a half. It would enter our atmosphere with a velocity of nearly 25,000 feet in a second; but the resistance of the air increasing with the velocity, would soon greatly reduce it, and render it uniform. We may remark, however, that all the accounts of fallen stones agree in attributing to the luminous bodies a rapid motion in the air, and the effects of a very considerable momentum to the fragments which reach the ground. The oblique direction in which they always fall, must tend greatly to diminish their penetrating power.

While we are investigating the circumstances that render this account of the matter highly probable, we ought not to omit one consideration, which lies wholly in the opposite scale. The greater part of these singular bodies have first appeared in a high state of ignition; and it does not seem easy to conceive how their passage through so rare a fluid as the atmosphere could have generated any great degree of heat, with whatever rapidity they may have moved. Viewing, as we do, the hypothesis of their lunar origin as by far the most probable in every other respect, we will acknowledge that this circumstance prevents us from adopting it with entire satisfaction. And while we see so many invincible objections to all the other theories which have been offered for the solution of the difficulty, we must admit that the supposition least liable to contradiction from the facts, is nevertheless sufficiently exceptionable, on a single ground, to warrant us in concluding with the philosophical remark of Vauquelin, "Le parti le plus sage qui nous reste à prendre dans cet état des choses, c'est d'avouer franchement, que nous ignorons entièrement l'origine de ces pierres, et les causes qui ont pu les produire."

If, however, a more extensive collection of accurate obser-

vations, and a greater variety of specimens, shall enable us to reconcile the discrepancy, and to push still farther our inquiries into the nature of the new substance, a knowledge of the internal structure of the moon may be the splendid reward of our investigations. And, while the labours of the astronomer and optician are introducing new worlds to our notice, Chemistry may, during the nineteenth century, as wonderfully augment our acquaintance with their productions and arrangement, as she has already, within a much shorter period, enlarged our ideas of the planet which we inhabit.*

* This Tract is a paper contributed to the Edinburgh Review, and printed in the number for January, 1804. The subject has since undergone much further investigation; and opinions are greatly changed upon it. This Tract was designed as a sifting of the evidence by which the facts are proved, and those facts resting on that evidence have formed the ground of all the subject investigates.—Note V.

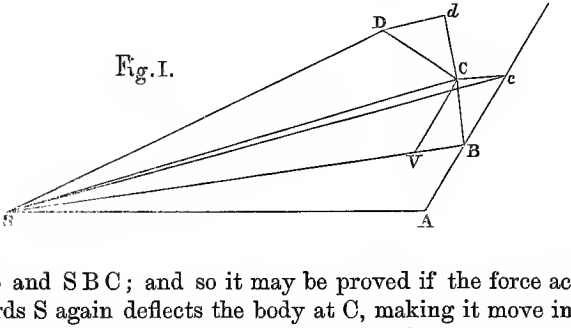
XI.

CENTRAL FORCES—AND LAW OF THE UNIVERSE ANALYTICALLY INVESTIGATED.

The fundamental proposition of the whole Newtonian system is this. If a body is driven by any single impulse or force of projection, and is also drawn continually by another force so as to revolve round a fixed centre, the radius vector, or line drawn from the body to that centre, describes areas which are in the same fixed plane, and are always proportional to the times of the body's motion; and conversely, if any body which moves in any curve described in a plane so that the radius vector to a point either fixed or moving uniformly in a straight line, describes areas proportional to the times of the body's motion, that body is acted on by a centripetal force tending towards and drawing it to the point.

To prove this, we have to consider that if a body moves equably on a straight line, the areas or triangles which are described by a line drawn from it to any point are proportional to the portions of the straight line through which the body moves (that is to the time, since, moving equably, it moves through equal spaces in equal times), because those triangles, having the same altitude, are to one another in the proportions of their bases. S being the point and A O the line of motion, S A B is to S B *c* as A B to B *c*. If then at B a force acts in the line S B, drawing the body towards S, it will move in the diagonal B C of a parallelogram of which the sides are B *c* and B V, the line through which the deflecting force would make it move if the motion caused by the other force ceased. C *c* therefore is parallel to V B, and the triangle

S B C is equal to the triangle S B c; consequently the motion through A B and B C, or the times, are as the two triangles

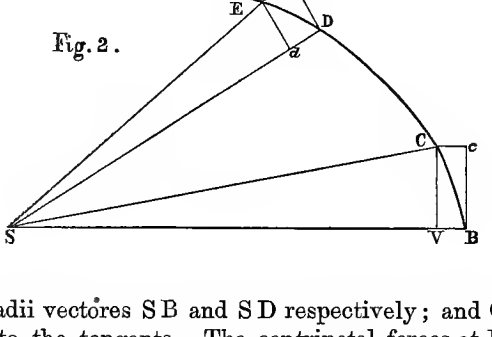


S A B and S B C; and so it may be proved if the force acting towards S again deflects the body at C, making it move in the diagonal C D. If, now, instead of this deflecting force acting at intervals A, B, C, it acts at every instant, the intervals of time become less than any assignable time, and then the spaces A B, B C, C D will become also indefinitely small and numerous, and they will form a curve line; and the straight lines drawn from any part of that curve to S will describe curvilinear areas, as the body moves in the curve A B C D, those areas being proportional to the times.—So conversely, if the triangles S B c and S B C are equal, they are between the same parallels, and c C is parallel to S B, and D d to S C; consequently the force which deflects acts in the lines S B and S C, or towards the point S.—It is equally manifest that the direction of the lines B c, C d, from which the centripetal force deflects the body, is that of tangents to the curve which the body describes, and that consequently the velocity of the body is in any given point inversely proportional to the perpendicular drawn from the centre to the tangent; the areas of the triangles whose bases are equal, being in the proportion of their altitude, that is, of those perpendiculars, and those areas being by the proposition, proportional to the times.

There are several other corollaries to this important proposition which deserve particular attention. B c and D e are

tangents to the curve at B and D respectively; B C and D E the arcs described in a given time; C c and E e lines parallel

Fig. 2.



to the radii vectores S B and S D respectively; and C V, E d parallel to the tangents. The centripetal forces at B and D must be in the proportion of V B and d D (being the other sides of the parallelograms of forces) if the arcs are evanescent, so as to coincide with the diagonals of the parallelograms V c and de. Hence the centripetal forces in B and D are as the versed sines of the evanescent arcs; and the same holds true if instead of two arcs in the same curve, we take two arcs in different but similar curves.*

From these propositions another follows plainly, and its consequences are most extensive and important. If two or more bodies move in circular orbits (or trajectories) with an equable motion, they are retained in those paths by forces tending towards the centres of the circles; and those forces are in the direct proportion of the squares of the arcs described in a given time, and in the inverse proportion of the radii of the circles.

First of all it is plain, by the fundamental proposition, that the forces tend to the centres S, s, because the sectors A S B and P B S being as the arcs A B, B P, and the sectors $a s b$,

* If B C, D E, are bisected, the proportion is found with the halves of V B, D d; and that is the same proportion with the whole versed sines.

$p\,b\,s$, as the arcs $a\,b$, $b\,p$, which arcs being all as the times, the areas are proportional to those times of describing them, and therefore S and s are the centres of the deflecting forces. Then, drawing the tangents A C, $a\,c$, and completing the parallelograms D C, $d\,c$, the diagonals of which coincide with the evanescent arcs A B, $a\,b$, we have the centripetal forces in A and a, as the versed sines A D, $a\,d$. But because A B P and $a\,b\,p$ are right angles (by the property of the circle), the triangles A D B, A P B, and $a\,d\,b$, $a\,p\,b$, are respectively similar

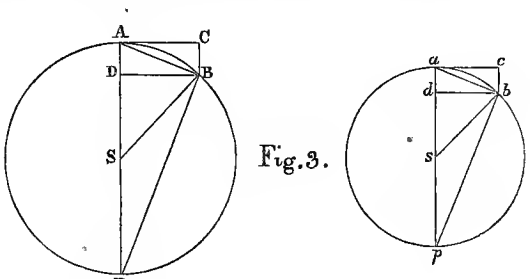


Fig. 3.

to one another. Wherefore A D : A B :: A B : A P and A D $= \frac{\text{A B}^2}{\text{A P}}$; and in like manner $a\,d = \frac{a\,b^2}{a\,p}$, or, as the evanescent arcs coincide with the chords, A D = arc $\frac{\text{A B}^2}{\text{A P}}$ and $a\,d$ = arc $\frac{a\,b^2}{a\,p}$. Now these are the properties of any arcs described in equal times; and the diameters are in the proportion of the radii; therefore the centripetal forces are directly as the squares of the arcs, and inversely as the radii.

It is difficult to imagine a proposition more fruitful in consequences than this; and therefore it has been demonstrated with adequate fulness.

In the *first* place, the arcs described being as the velocities, if F, f are the centripetal forces, and V, v the velocities, and R, r the radii, F : f :: V^2 : v^2; and also :: r : R; or F : f ::

$\frac{V^2}{R} : \frac{v^2}{r}$. Now as in the circle V and R, v and r are both constant quantities, the contripetal force is itself constant, which retains a body by deflecting it towards the centre of the circle.

Secondly. The times in which the whole circles are described (called the periodic times) are as the total circumferences or peripheries; $T : t :: P : p$. But the peripheries are as the radii or $:: R : r$. Therefore $T : t :: R : r$; also $V : v :: \frac{P}{T} : \frac{p}{t}$, therefore inversely as the radii, or $T : t :: \frac{R}{V} : \frac{r}{v}$, and $V^2 : v^2 :: \frac{R^2}{T^2} : \frac{r^2}{t^2}$. But the centripetal forces $F : f :: \frac{V^2}{R} : \frac{v^2}{r}$; substituting for the ratio of $V^2 : v^2$, its equal the ratio of $\frac{R^2}{T^2} : \frac{r^2}{t^2}$, $F : f :: \frac{R}{T^2} : \frac{r}{t^2}$; or the centripetal forces are directly as the distances and inversely as the squares of the periodic times; the forces being as the distances if the times are equal; and the times being equal if the forces are as the distances.—It also follows that if the periodic times are as the distances, then $F : f :: \frac{R}{R^2} : \frac{r}{r^2}$; that is, $:: \frac{1}{R} : \frac{1}{r}$, or inversely as the distances.—In like manner if the periodic times are in proportion to any power n, of the distance, or $T : t :: R^n : r^n$, we shall have $T^2 : t^2 :: R^{2n} : r^{2n}$ and $F : f :: \frac{R}{R^{2n}} : \frac{r}{r^{2n}}$; that is $:: \frac{1}{R^{2n-1}} : \frac{1}{r^{2n-1}}$; and conversely if the centripetal force is in the inverse ratio of the $(2n-1)^{\text{th}}$ power of the distance, the periodic time is as the n^{th} power of that distance.—Likewise, as the velocities of the bodies in their orbits or $V : v :: \frac{R}{T} : \frac{r}{t}$,

if we make $T : t :: R^n : r^n$, then $V : v :: \frac{R}{R^n} : \frac{r}{r^n}$, or $:: \frac{1}{R^{n-1}} : \frac{1}{r^{n-1}}$. Thus, suppose n is equal to $\frac{3}{2}$ we have for the velocities $V : v :: \frac{1}{\sqrt{R}} : \frac{1}{\sqrt{r}}$, or they are in the inverse subduplicate proportion of the distances; and for the centripetal forces we have $F : f :: \frac{1}{R^{3-1}} : \frac{1}{r^{3-1}} :: \frac{1}{R^2} : \frac{1}{r^2}$; or the attraction to the centre is inversely as the square of the distance.

Now if $n = \frac{3}{2}$, $T : t :: R^{\frac{3}{2}} : r^{\frac{3}{2}}$, or $T^2 : t^2 :: R^3 : r^3$; in other words the squares of the periodic times are as the cubes of the distances from the centre, which is the law discovered by Kepler from observation actually to prevail in the case of the planets. And as he also showed from observation that they describe equal areas in equal times by their radii vectores drawn to the sun, it follows from the fundamental proposition, *first*, that they are deflected from the tangents of their orbits by a power tending towards the sun; and then it follows, *secondly*, from the last deduction respecting it, (namely, the proportion of $F : f :: \frac{1}{R^2} : \frac{1}{r^2}$,) that this central force acts inversely as the squares of the distances, always supposing the bodies to move in circular orbits, to which our demonstration has hitherto been confined.*

The extension, however, of the same important proposition to the motion of bodies in other curves is easily made, that is to the motion of bodies in different parts of the same curve or in curves which are similar. For in evanescent portions of the same curve, the osculating circle, or circle which has the same curvature at any point, coincides with the curve at that point; and if a line is drawn to the extremity of that

* This sesquiplicate proportion only holds true on the supposition of the bodies all moving without exerting any action on each other.

circle's diameter, A M B and $a\ m\ b$ may be considered as triangles; and as they are right angled at M and m, A M^2 is equal to A P × A B and $a\ m^2$ to $a\ p \times a\ b$; and where the

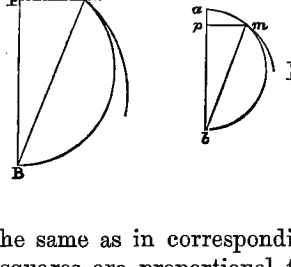


Fig. 4.

curvature is the same as in corresponding points of similar curves, those squares are proportional to the lines A P, or $a\ p$; or those versed sines of the arcs A M and $a\ m$ are proportional to the squares of the small arcs. Hence if the distances of two bodies from their respective centres of force be D, d, the deflecting force in any points A and a, being as the versed sines, those forces are as A $M^2 : a\ m^2$; and from hence follows generally in all curves, that which has been demonstrated respecting motion in circular orbits.

The planets then and their satellites being known by Kepler's laws to move in elliptical orbits, and to describe round the sun in one focus areas proportional to the times by their radii vectores drawn to that focus, and it being further found by those laws that the squares of their periodic times are as the cubes of the mean distances from the focus, they are by these propositions of Sir Isaac Newton which we have been considering, shown to be deflected from the tangent of their orbit, and retained in their paths, by a force acting inversely as the squares of the distances from the centre of motion.

But another important corollary is also derived from the same proposition. If the projectile or tangential force in the direction A T ceases (next figure), the body, instead of moving in any arc A N, is drawn by the same centripetal force in the straight line A S. Let A n be the part of A S,

through which the body falls by the force of gravity, in the same time that it would take to describe the arc A N. Let A M be the infinitely small arc described in an instant; and A P its versed sine. It was before shown, in the corollaries to the first proposition, that the centripetal force in A is as A P, and the body would move by that force through A P, in the same time in which it describes the arc A M. Now the force of gravity being one which operates like the centripetal force at every instant, and uniformly accelerates the descending body, the spaces fallen through will be as the squares of the times. Therefore, if A n is the space through which the body falls in the same time that it would describe A N, A P

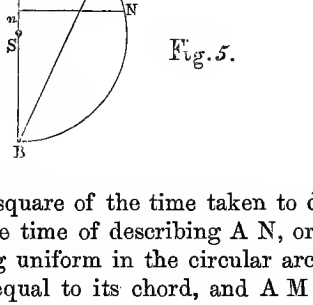


Fig. 5.

is to A n as the square of the time taken to describe A M to the square of the time of describing A N, or as $AM^2 : AN^2$, the motion being uniform in the circular arc. But A M, the nascent arc, is equal to its chord, and A M B being a right angled triangle as well as A P M, A B : A M :: A M : A P and $AP = \frac{AM^2}{AB}$. Substituting this in the former proportion, we have $\frac{AM^2}{AB} : An :: AM^2 : AN^2$, or $An : AN^2 :: \frac{AM^2}{AB} : AM^2$, that is :: 1 : A B. Therefore $AN^2 = An \times AB$, or the arc described, is a mean proportional between the diameter of the orbit, and the space through which the body would fall by gravity alone, in the same time in which it describes the arc.

Now let A M N B represent the orbit of the moon; A N the arc described by her in a minute. Her whole periodic time is found to be 27 days 7 hours and 43 minutes, or 39,343 minutes; consequently A N : 2 A N B :: 1 : 39,343.

But the mean distance of the moon from the earth is about 30 diameters of the earth, and the diameter of her orbit, 60 of those diameters; and a great circle of the earth being about 131,630,572 feet, the circumference of the moon's orbit must be 60 times that length, or 7,897,834,320, which being divided by 39,343 (the number of minutes in her periodic time), gives for the arc A N described in one minute 200,743, of which the square is 40,297,752,049, or $A N^2$, which (by the proposition last demonstrated) being divided by the diameter A B gives A n. But the diameter being to the orbit as 1 : 3.14159 nearly, it is equal to about 2,513,960,866. Therefore A n = 16.02958, or 16 feet, and about the third of an inch. But the force which deflects the moon from the tangent of her orbit, has been shown to act inversely as the square of the distance; therefore she would move 60 × 60 times the same space in a minute at the surface of the earth. But if she moved through so much in a minute, she would in a second move through so much less in the proportion of the squares of those two times, as has been before shown. Wherefore she would in a second move through a space equal to $16\frac{1}{3\frac{1}{4}}$ nearly (16.02958). But it is found by experiments frequently made, and among others by that of the pendulum,* that a body falls about this space in one second upon the surface of the earth. Therefore the force which deflects the moon from the tangent of her orbit, is of the same amount, and acts in the same direction, and follows the same proportions to the time that gravity does. But if the moon is drawn by any other force, she must also be drawn by gravity;

* It is found that a pendulum, vibrating seconds, is about the length of 3 feet $3\frac{1}{3}$ inches in this latitude; and the space through which a body falls in a second is to half this length as the square of the circumference of a circle to that of the diameter, or as 9.8695 : 1, and that is the proportion of the half of 3 feet $3\frac{1}{3}$ inches to somewhat more than 16 feet.

and as that other force makes her move towards the earth 16 feet $\frac{1}{3}$ inch, and gravity would make her move as much, her motion would therefore be 32 feet $\frac{2}{3}$ inch in a second at the earth's surface, or as much in a minute in her orbit; and her velocity in her orbit would therefore be double of what it is, or the lunar month would be less than 13 days and 16 hours. It is, therefore, impossible that she can be drawn by any other force, except her gravity, towards the earth.*

Such is the important conclusion to which we are led from this proposition, that the centripetal forces are as the squares of the arcs described directly, and as the distances inversely. This conclusion was the discovery of the great law of the universe. The fruit of the consequences of this proposition is the ascertaining the laws of curvilinear motion generally.

The versed sine of the half of any evanescent arc (or sagitta of the arc) of a curve in which a body revolves, was proved to be as the centripetal force, and as the square of the times; or as $F \times T^2$. Therefore the force F is directly as the versed sine, and inversely as the square of the time. From this it follows that the central force may be measured in several ways. The arc being Q C, we are to measure the central force in its middle point P. Then the areas being as the times; twice the triangle S P Q, or Q L $\times$ S P is as T in the last expression; and, therefore, Q R being parallel to L P, the central force at P is as $\frac{Q R}{S P^2 \times L Q^2}$. So if S Y be the perpendicular upon the tangent P Y, because P R and the arc P Q, evanescent, coincide, twice the triangle S P Q is equal to S Y $\times$ Q P; and the central force in P is as $\frac{Q R}{S Y^2 \times Q P^2}$. Lastly, if the revolution be in a circle, or in a curve having at P the same curvature with a circle whose chord passes from that

* The proposition may be demonstrated by means of the Prop. XXXVI. of Book I. of the Principia, as well as by means of the proposition of which we have now been tracing the consequences (Prop. IV.). But in truth the latter theorem gives a construction of the former problem (Prop. XXXVI.), and from it may be deduced both that and Prop. XXXV.

point through S to V, then the measure of the central force will be $\frac{1}{SY^2 \times PV}$.—By finding the value of those solids in

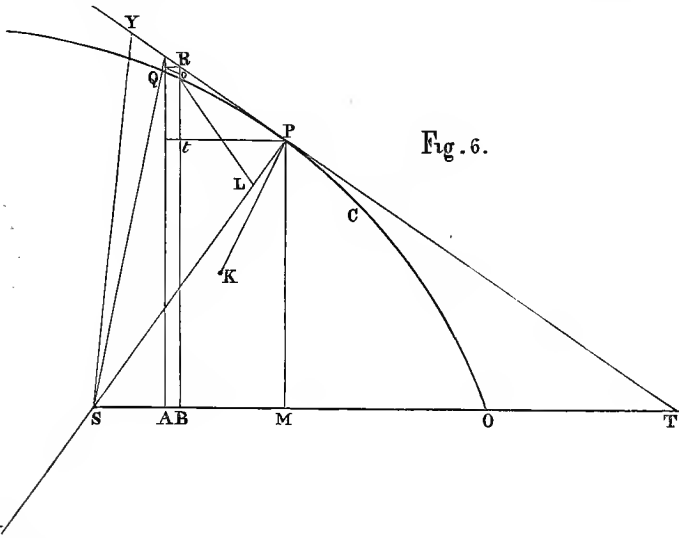


Fig. 6.

any given curve, we can determine the centripetal force in terms of the radius vector SP; that is, we can find the proportion which the force must bear to the distance, in order to retain the body in the given orbit or trajectory; and conversely, the force being given, we can determine the trajectory's form.

This proposition, then, with its corollaries, is the foundation of all the doctrine of centripetal forces, whether direct or inverse; that is, whether we regard the method of finding, from the given orbit, the force and its proportion to the distance, or the method of finding the orbit from the given force. We must, therefore, state it more in detail, and in the analytical manner, Sir Isaac Newton having delivered it synthetically, geometrically, and with the utmost brevity.

It may be reduced to five kinds of formulæ.

1. If the central force in two similar orbits be called F and f, the times T and t, the versed sines of half the arcs S and s,—then $F : f :: \frac{S}{T^2} : \frac{s}{t^2}$; and generally F is as $\frac{S}{T^2}$.

2. But draw S P to any given point of the orbit in the middle of an infinitely small arc Q C. Let T P touch the curve in P, draw the perpendicular S Y from the centre of forces S to P T produced, draw S Q infinitely near S P, and Q R parallel to S P, Q o and R o parallel to the co-ordinates S M, M P. Then P being the middle of the arc, twice the triangle S P Q is proportional to the time in which C Q is described. Therefore Q P × S Y or Q L × P S is proportional to the time; and Q R is the versed sine of $\frac{C Q}{2}$, therefore F as $\frac{S}{T^2}$ becomes F as $\frac{Q R}{L Q^2 \times S P^2}$; and if S M = x, M P = y, and because the similar triangles Q R o and S M P give $Q R = \frac{Q o \times S P}{S M}$, and because A M being the first differential of S M, o Q is its second differential (negatively), therefore $Q R = \frac{-d^2 x \times \sqrt{x^2 + y^2}}{x}$ (taken with reference to dt constant), and F is as $\frac{-d^2 x \sqrt{x^2 + y^2}}{x \times L Q^2 \times (x^2 + y^2)}$. But $L Q^2 = Q P^2 - L P^2$ and L P is the differential of S P or $\sqrt{x^2 + y^2}$. Therefore $L Q^2 = \frac{(x\,dy - y\,dx)^2}{x^2 + y^2} = \frac{y^4 \left(d \frac{x}{y}\right)^2}{x^2 + y^2}$, and F is as $\frac{-d^2 x \sqrt{x^2 + y^2}}{x y^4 \left(d \frac{x}{y}\right)^2}$.

But as the differential of the time (L Q × P S) may be made constant, Q R will represent the centripetal force; and

that force itself will therefore be as $-\frac{d^2x\sqrt{x^2+y^2}}{x}$,* taken with reference to dt constant.

3. The rectangle S Y × Q P being equal to Q L × S P and $SY = \frac{y\,dx - x\,dy}{\sqrt{dx^2+dy^2}}$, we have F as $\frac{QR}{SY^2 \times QP^2} = \frac{QR}{(y\,dx - x\,dy)^2} = \frac{QR}{y^4\left(d\frac{x}{y}\right)^2}$.

4. Because $F = \frac{QR}{SY^2 \times QP^2}$ and $\frac{QP^2}{QR}$ is equal to the chord P V of the circle, which has the same curvature with Q P O in P, and whose centre is K (and because $QP^2 = QR \times PV$ by the nature of the circle and the equality of the evanescent arc Q P with its sine, and thus $PV = \frac{QP^2}{QR}$, — therefore $\frac{QR}{QP^2} = \frac{1}{PV}$), F is as $\frac{1}{SY^2 \times PV}$. In like manner if the velocity, which is inversely as S Y, be called v, F is as $\frac{v^2}{PV}$. Now the chord of the osculating circle is to twice the perpendicular S Y as the differential of S P to the differential of the perpendicular; and calling S P the radius vector r, and $SY = p$, we have $PV = \frac{2p\,dr}{dp}$, and F is as $\frac{dp}{2p^3\,dr}$; and also F is as $\frac{v^2\,dp}{2\,dr}$. In these formulas, substituting for p and r their

* Of these expressions, although I have sometimes found this, which was first given by Herrman, serviceable, I generally prefer the two, which are in truth one, given under the next heads. But the expression first given $-\frac{d^2x\sqrt{x^2+y^2}}{xy^4\left(d\frac{x}{y}\right)^2}$ is without integration an useful one.

values in terms of x and y, we obtain a mean of estimating the force as proportioned to r, which is $\sqrt{x^2 + y^2}$.

5. The last article affords, perhaps, the most obvious methods of arriving at central forces, both directly and inversely. Although the quantities become involved and embarassing in the above general expressions for all curves, yet in any given curve the substitutions can more easily be made. A chief recommendation of these expressions is, that they involve no second differentials, nor any but the first powers of any differentials. But it may be proper to add other formulas which have been given, and one of which, at least, is more convenient than any of the rest.

One expression for the centrifugal force (and one sometimes erroneously given for the centripetal)* is $\frac{d\,s^2}{2\,\mathrm{R}}$, s being the length of the curve and R the radius of curvature. This gives the ready means of working if that radius is known. But its general expression involves second differentials, the usual formula for it being $\frac{d\,s^3}{d\,x^2 \times d\left(\frac{d\,y}{d\,x}\right)}$; consequently we must first find $\frac{d\,y}{d\,x} = \mathrm{X}$ (a function of x), and then there are only first differentials.

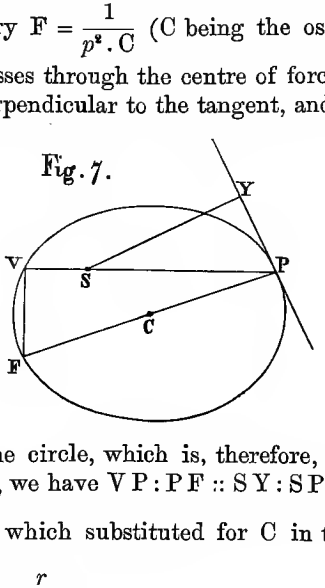
Another for this radius of curvature is $\frac{d\,s^2}{\sqrt{(d^2 y)^2 + (d^2 x)^2}}$, and this is used by Laplace; and another is $\frac{r\,d\,r}{d\,p}$, which, with other valuable formulas, is to be obtained from Maclaurin's Fluxions. But the formula generally ascribed to John Ber-

* This error appears to have arisen from taking the case where the radius of curvature and radius vector coincide, that is, the case of the circle, in which the centrifugal and centripetal forces are the same.—See Mrs. Somerville's truly admirable work on the Mec. Cel., where the error manifestly arises from this circumstance.

noulli (Mém. Acad. des Sciences, 1710), is, perhaps, the most elegant of any, $F = \frac{r}{2 . p^3 \times R}$; and this results from substituting 2 R for its value $\frac{2 r dr}{dp}$, in the equation to F, deduced above from Newton's formula, namely, $F = \frac{dp}{2 p^3 dr}$.

But the proposition is so important, that it may be well to prove it, and to show that it is almost in terms involved in the third corollary to Prop. VI. Book I. of the Principia.—By that corollary $F = \frac{1}{p^2 . C}$ (C being the osculating circle's chord which passes through the centre of forces). But drawing S Y, the perpendicular to the tangent, and P C F through

Fig. 7.



the centre of the circle, which is, therefore, parallel to Y S, and joining V F, we have V P : P F :: S Y : S P or C : 2 R :: p : r and $C = \frac{2 R . p}{r}$, which substituted for C in the above equation, gives $F = \frac{r}{2 P^3 . R}$.

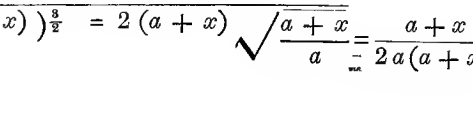
In all these cases p is to be found first, and the expression for it (because, pp. 286, 287, T P : P M :: T S : S Y an $TS = \frac{y dx - x dy}{dy}$, and $PT = \frac{y}{dy} \sqrt{dy^2 + dx^2}$) is $p = SY$

$= \dfrac{y\,dx - x\,dy}{\sqrt{dy^2 + dx^2}} = \dfrac{y^2 d\dfrac{x}{y}}{\sqrt{dy^2 + dx^2}}$. Also $r = \mathrm{SP} = \sqrt{x^2 + y^2}$.

Then the radius of curvature $\mathrm{R} = \dfrac{(dx^2 + dy^2)^3}{dx^2 \times d\mathrm{X}}$ (X being $\dfrac{dy}{dx}$ in terms of x, and having no differential in it when the substitution for dy is made). Therefore, the expression for the centripetal force becomes $\dfrac{\sqrt{x^2 + y^2} \times dx^2 \times d\mathrm{X}}{2y^4\left(d\dfrac{x}{y}\right)^2}$, in which, when y and dy are put in terms of x, as both numerator and denominator will be multiplied by dx^3, there will be no differential, and the force may be found in terms of the radical—that is, of r, though often complicated with x also. It is generally advisable, having the equation of the curve, to find p, r, and R, first by some of the above formulas, and then substitute those values, or dp and dr, in either of the expressions for F, $\dfrac{dp}{2p^3 dr}$ or $\dfrac{r}{2p^3 \mathrm{R}}$.

To take an example in the parabola, where S being the focus, and $\mathrm{O\,S} = a$, $y^2 = 4ax$, and $\mathrm{T\,M} = 2x$, and $p = \mathrm{Y\,S} = \sqrt{(a + x)\,a}$; $r = \mathrm{S\,P} = a + x$, and $\mathrm{R} = \dfrac{r\,dr}{dp} = 2(a + x)\sqrt{\dfrac{a + x}{a}}$; we have therefore F as $\dfrac{r}{p^3 \cdot \mathrm{R}} =$

Fig. 8.



$\dfrac{a + x}{(a(a + x))^{\frac{3}{2}}} = 2(a + x)\sqrt{\dfrac{a + x}{a}} = \dfrac{a + x}{2a(a + x)^3} =$

$\frac{1}{2a(a+x)^2} = \frac{1}{2 \cdot OS \cdot SP^2}$, or, because OS (the parameter) is constant, inversely as the square of the distance: And the other formula F as $\frac{dp}{p^3 dr}$ gives the same result for the law of force, or $\frac{1}{4 S P^2}$.*

Again, in the ellipse, if a be half the transverse axis, and b half the conjugate, and r the radius vector, we have $p = b\sqrt{\frac{r}{2a-r}}$, and $dp = \frac{a b dr}{\sqrt{r}(2a-r)^{\frac{3}{2}}}$; therefore the formula $\frac{dp}{p^3.dr}$ becomes $\frac{a b dr}{b^2 \sqrt{r} \times r^{\frac{3}{2}} \times dr} = \frac{a}{b^2 r^2}$, or the force is inversely as the square of the distance.

Lastly, as the equations are the same for the hyperbola, with only the difference of the signs, the value of the force is also inversely as r^2, or the square of the distance. In the circle a = the radius $= r = p$; hence $\frac{r}{p^3 R}$ becomes $\frac{a}{a^4}$, which, being constant, the force is everywhere the same. But if the centre of forces is not that of the circle, but a point in the circumference, the force is as $\frac{1}{r^5}$.

Respecting centrifugal forces it may be enough to add, that if v is the velocity and r the radius, the centrifugal force f, in a circle, is as $\frac{v^2}{r}$. Also if R be the radius of curvature, f for any curve is $= \frac{v^2}{R}$. When a body moves in a circle by a centripetal force directed to the centre, the centrifugal force

* This result coincides with the synthetical solution of Sir Isaac Newton in Prop. XIII.

is equal and opposite to the centripetal. Also the velocity in uniform motion, like that in a circle, being as $\frac{s}{t}$, the space divided by the time, and the arc being as the radius r, f is as $\frac{s^2}{r \cdot t^2}$ or as $\frac{r}{t^2}$. If two bodies moving in different circles have the same centrifugal force, then the times are as $\sqrt{r}$.—It is to the justly celebrated Huygens that we owe the first investigation of centrifugal forces. The above propositions, except the second, are abridged from his treatise.*

i. First, where the centre of force is the centre of the trajectory. In exemplifying the use of the formulas we have shown the proportion of the force to the distance in the conic sections generally, their foci being the centres of forces. Let us now see more in detail what the proportion is for the circle. Let S be the centre of forces and K of the circle, PT a tangent, SY a perpendicular to it, KM and MP co-ordi-

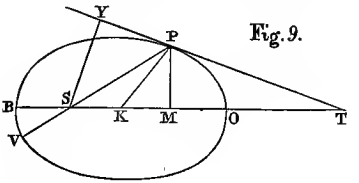


Fig. 9.

nates, SK = b, KO = a, PM = y, and MK = x. Then, by similar triangles, TKP and TSY, we have $SY = \frac{ST \times KP}{TK}$, or (because the sub-tangent $MT = \frac{y^2}{x}$, and $a^2 = x^2 + y^2$) $\frac{a^2 + bx}{a}$ or $\left(\frac{2a^2 + 2bx}{2a}\right)$; also $SP = \sqrt{a^2 + 2bx + b^2}$, and

* Horologium Oscillatorium, ed. 1673, p. 159, App.

because by the property of the circle $OS \times SB$ or $(a+b)(a-b) = a^2 - b^2 = PS \times SV$; therefore

$$SV = \frac{a^2 - b^2}{\sqrt{a^2 + 2bx + b^2}} \text{ and } PV = \frac{2a^2 + 2bx}{\sqrt{a^2 + 2bx + b^2}}.$$

Now by the formula already stated as Bernoulli's, but really Sir Isaac Newton's, the centripetal force in P is as $\frac{SP}{SY^3 \times R}$, R being the radius of curvature, and in the circle that is constant being $= a$, the semidiameter; therefore the force is as $\frac{\sqrt{a^2 + 2bx + b^2}}{\frac{a(2a^2 + 2bx)^3}{8a^3}}$, or as $\frac{8 \times a^2\sqrt{a^2 + 2bx + b^2}}{(2a^2 + 2bx)^3}$, that is $\frac{BO^2 \times SP}{(2a^2 + 2bx)^3}$; or as $\frac{BO^2 \times SP^3}{(2a^2 + 2bx)^3 \times SP^2}$, or as $\frac{BO^2}{\frac{(2a^2 + 2bx)^3}{SP^3} \times PS^2}$. But $\frac{2a^2 + 2bx}{SP} = \frac{2a^2 + 2bx}{\sqrt{a^2 + 2bx + b^2}}$ $= PV$. Therefore the central force is as $\frac{BO^2}{PV^3 \times SP^2}$; or (because OB^2 is constant) the central force is inversely as the square of the distance and the cube of the chord jointly. Of consequence, where S is in the centre of the circle and $b = o$, the force is constant, SP becoming the radius and PV the diameter; and if S is in the circumference of the circle as at B, or $a = b$, then the chord and radius vector coinciding, the force is inversely as the fifth power of the distance, and is also inversely as the fifth power of the cosine of the angle PSO.

By a similar process it is shown that in an ellipse the force directed to the centre is as the distance. Indeed, a property of the ellipse renders this proof very easy. For if SY is the perpendicular to the tangent TP, and NP (the normal)

parallel to S Y, and S A the semi-conjugate axis; S A is a mean proportional between S Y and P N, and therefore S Y = $\frac{A S^2}{P N}$; also the radius of curvature of the ellipse is (like that

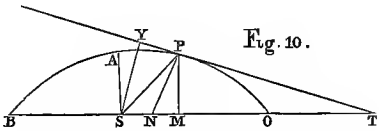


Fig. 10.

of all conic sections) equal to $\frac{4 P N^3}{P^2}$, P being the parameter.

Therefore we have to substitute these values for S Y and the radius of curvature, R, in the expression for the central force, $\frac{S P}{R \times S Y^3}$, and we have $\frac{S P}{\frac{4 . P N}{P^2} \times \frac{A S^6}{P N^3}} = \frac{P^2}{4 A S^6} \times S P$; so that, neglecting the constant $\frac{P^2}{4 A S^6}$, the centripetal force is as the distance directly.

From hence it follows, conversely, that if the centripetal force is as the distance, the orbit is elliptical or circular, for by reversing the steps of the last demonstration we arrive at an equation to the ellipse; or, in case of the two axes being equal, to the circle. It also follows that if bodies revolve in circular or elliptical orbits round the same centre, the centre of the figures being the centre of forces, and the force being as the distance, the periodic time of all the bodies will be the same, and the spaces through which they move, however differing in length from each other, will all be described in the same time. This proposition, which sometimes has appeared paradoxical to those who did not sufficiently reflect on the subject, is quite evident from considering that the force and velocity being increased in proportion to the distance, and

the lengths of similar curvilinear and concentric figures being in some proportion, and that always the same, to the radii, the lengths are to each other as those radii, and consequently the velocity of the whole movement is increased in the same proportion with the space moved through. Hence the times taken for performing the whole motion must be the same. Thus, if V and v are the velocities, R and r the radii, S and s the lines described in the times T and t, by two such bodies round a common centre, $V : v :: R : r$, and $S : s :: R : r$; and because $V = \frac{S}{T}$ and $v = \frac{s}{t}$, $\frac{S}{T} : \frac{s}{t} :: R : r$, and $S : s :: TR : tr$; or $R : r :: TR : tr$; and therefore $T = t$. Hence if gravity were the same towards the sun that it is between the surface and centre of each planet, or if the sun were moved but a very little to one side, so as to be in the centre of the ellipse, the whole planets would revolve round him in the same time, and Saturn and Uranus would, like Mercury, complete their vast courses in about three of our lunar months instead of 30 and 80 years—a velocity in the case of Uranus equal to 75,000 miles in a second, or nearly one-third that of light.

It also follows from this proposition that, if such a law of attraction prevailed, all bodies descending in a straight line to the centre would reach it in the same time from whatever distance they fell, because the elliptic orbit being indefinitely stretched out in length and narrowed till it became a straight line, bodies would move or vibrate in equal times through that line. This is the law of gravity at all points within the earth's surface.

Another consequence of this proposition is, that if the centre of the ellipse be supposed to be removed to an infinite distance, and the figure to become a parabola, the centripetal force being directed to a point infinitely remote, becomes constant and equable; a proposition discovered first by Galileo.

Next, where the centre of force is in the focus. If O P A be a conic section whose parameter is D, S Y the perpendicular to the tangent T P, P R the radius of curvature at P; then $S Y : S P :: \frac{1}{2} D : P N$ (the normal), and $S Y = \frac{D \cdot S P}{2 \cdot P N}$; also $P R = \frac{4 P N^3}{D^2}$. Substitute these values of S Y and P R (p and R) in the expression formerly given for the central

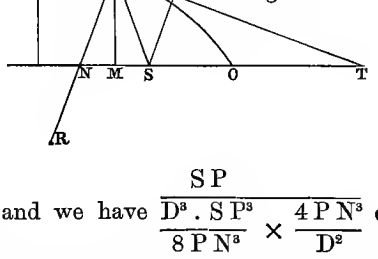


Fig. 11.

force $\frac{r}{p^3 \times R}$, and we have $\frac{S P}{\frac{D^3 \cdot S P^3}{8 P N^3} \times \frac{4 P N^3}{D^2}}$ or $\frac{2}{D \times S P^2}$, which is (D being invariable) as the inverse square of the distance. Therefore any body moving in any of the conic sections by a force directed to the focus, is attracted by a centripetal force inversely as the square of the distance from that focus. This demonstration, therefore, is quite general in its application to all the conic sections.

It follows that if a body is impelled in a straight line with any velocity whatever, from an instantaneous force, and is at the same time constantly acted upon by a centripetal force which is inversely as the square of the distance from the centre, the path which the body describes will be one or other of the conic sections. For if we take the expression $\frac{1}{D \cdot S P^2}$ and work backwards, multiplying the numerator and denominator both by S P, and then multiplying the denominator

by $\frac{8\,D^2 \,.\, P\,N^3}{8\,D^2 \,.\, P\,N^3}$, we obtain the expressions for the value of S Y, the perpendicular, and for R, the radius of curvature. But no curves can have the same value of S Y and R, except the conic sections; because there are no other curves of the second order, and those values give quadratic equations between the co-ordinates.

By pursuing another course of the same kind algebraically, we obtain an equation to the conic sections generally, according as certain constants in it bear one or other proportion to one another, The perpendicular S Y and the radius of curvature are given in terms of the normal; and either one or the other will give the equation. Thus $R = \frac{(d\,x^2 + d\,y^2)^{\frac{3}{2}}}{d\,x^2 \times d\left(\frac{d\,y}{d\,x}\right)}$ $= \frac{4\,P\,N^3}{D^2} = \frac{4\,y^3}{D^2\,d\,x^3} \times (d\,x^2 + d\,y^2)^{\frac{3}{2}}$ which gives $D^2\,d\,x^3 = 4\,y^3 \times (d^2\,y\,d\,x - d^2\,x\,d\,y)$ an equation to the co-ordinates. Now whether this be resolvable or not, it proves that only one description of curves, of one order, can be such as to have the property in question. The former operation of going back from the expression of the central force, proves that the conic sections answer this condition. Therefore no other curves can be the trajectories of bodies moving by a centripetal force inversely as the square of the distance.*

This truth, therefore, of the necessary connexion between motion in a conic section and a centripetal force inversely as the square of the distance from the focus, is fully established by rigorous demonstration of various kinds.

* The equation may be resolved and integrated; there results, in the first instance, the equation $d\,x = \frac{2\,y\,d\,y}{\sqrt{2\,c\,y^2 - D^2}}$, and therefore the integral is this quadratic, $c^2\,x^2 - 2\,c\,y^2 - 2\,c\,C\,x + C^2 + D^2 = 0$.

If we now compare the motion of different bodies in concentric orbits of the same conic sections, we shall find that the areas which, in a given time, their radii vectores describe round the same focus, are to one another in the subduplicate ratio of the parameters of those curves. From this it follows, that in the ellipse whose conjugate axis is a mean proportional between its transverse axis and parameter, the whole time taken to revolve (or the periodic time) being in the proportion of the area (that is in the proportion of the rectangle of the axes) directly, and in the subduplicate ratio of the parameter inversely, is in the sesquiplicate ratio of the transverse axis, and equal to the periodic time in a circle whose diameter is that axis. It is also easy to show from the formula already given respecting the perpendicular to the tangent, that the velocities of bodies moving in similar conic sections round the same focus, are in the compound ratio of the perpendiculars inversely and the square roots of the parameters * directly. Hence in the parabola a very simple expression obtains for the velocity. For the square of the perpendicular being as the distance from the focus by the nature of the curve (the former being $a^2 + a\,x$, and the latter $a + x$), the velocity is inversely as the square root of that distance. In the ellipse and hyperbola where the square of the perpendicular varies differently in proportion to the distance, the law of the velocity varies differently also. The square of the perpendicular in the ellipse (A being the transverse axis and B the conjugate, and r the radius vector) is $\frac{B^2 \times r}{A - r}$; in the hyperbola, $\frac{B^2 \times r}{A + r}$, or those squares of the perpendicular vary as $\frac{r}{A - r}$ and $\frac{r}{A + r}$, in those curves respectively, B^2 being constant. Hence the velocities of bodies moving in the former curve vary in a greater ratio than that

* By parameter is always to be understood, unless otherwise mentioned, the principal parameter, or the parameter to the principal diameter.

of the inverse subduplicate of the distance, or $\frac{1}{\sqrt{r}}$, and in a smaller ratio in the latter curve, while in the parabola $\frac{1}{\sqrt{r}}$ is their exact measure.

To these useful propositions, Demoivre added a theorem of great beauty and simplicity respecting motion in the ellipse. The velocity in any point P is to the velocity in T, the point where the conjugate axis cuts the curve, as the square root of the line joining the former point P and the more distant focus, is to the square root of the line joining P and the nearer focus. It follows from these propositions that in the ellipse, the conjugate axis being a mean proportional between the transverse and the parameter, and the periodic time being as the area, that is as the rectangle of the axes directly, and the square root of the parameter inversely, t being that time, a and b the axes, and p the parameter, $t = \frac{a\,b}{\sqrt{p}}$, and $b^2 = a\,p$; therefore $a\,b = a\sqrt{a\,p} = \sqrt{a^3} \times \sqrt{p}$; and $t = \sqrt{a^3}$, and $t^2 = a^3$; or the squares of the periodic times are as the cubes of the mean distances. So that all Kepler's three laws have now been demonstrated, *à priori*, as mathematical truths; *first*, the areas proportional to the times if the force is centripetal—*second*, the elliptical orbit,—and *third*, the sesquiplicate ratio of the times and distances, if the force is inversely as the squares of the distances, or in other words if the force is gravity.

Again, if we have the velocity in a given point, the law of the centripetal force, the absolute quantity of that force in the point, and the direction of the projectile or centrifugal force, we can find the orbit. The velocity in the conic section being to that in a circle at the given distance D as m to n, and the perpendicular to the tangent being p, the lesser axis will be $\frac{2\,m\,p}{\sqrt{2\,n^2 - m^2}}$, and the greater axis $\frac{2\,\mathrm{D}\,n^2}{2\,n^2 - m^2}$, the

signs being reversed in the denominator of each quantity for the case of the hyperbola. Hence the very important conclusion that the length of the greater axis does not depend at all upon the direction of the tangential or projectile force, but only upon its quantity, the direction influencing the length of the lesser axis alone.

Lastly, it may be observed, that as these latter propositions give a measure of the velocity in terms of the radius vector and perpendicular to the tangent for each of the conic sections, we are enabled by knowing that velocity in any given case where the centripetal force is inversely as the square of the distance, and the absolute amount of that force is given, as well as the direction of the projectile force and the point of the projection, to determine the parameters and foci of the curve, and also which of the conic sections is the one described with that force. For it will be a parabola, an hyperbola, or an ellipse, according as the expression obtained for p^2 (the square of the perpendicular to the tangent) is as the radius vector, or in a greater proportion, or in a less proportion. This is the problem above referred to, which John Bernoulli had entirely overlooked, when he charged Sir Isaac Newton with having left unproved the important theorem respecting motion in a conic section, which is clearly involved in its solution.

Before leaving this proposition, it is right to observe that the two last of its corollaries give one of those sagacious anticipations of future discovery which it is in vain to look for anywhere but in the writings of Newton. He says, that by pursuing the methods indicated in the investigation, we may determine the variations impressed upon curvilinear motion by the action of disturbing, or, what he terms, foreign forces; for the changes introduced by these in some places, he says, may be found, and those in the intermediate places supplied, by the analogy of the series. This was reserved for Lagrange and Laplace, whose immortal labours have reduced the theory of disturbed motion to almost as great

certainty as that of untroubled motion round a point by virtue of forces directed thither.*

We have thus seen how important in determining all the questions, both direct and inverse, relating to the centripetal force, are the perpendicular to the tangent and the radius of curvature. Indeed it must evidently be so, when we consider, *first*, that the curvature of any orbit depends upon the action of the central force, and that the circle coinciding with the curve at each point, beside being of well-known properties, is the curve in which at all its points the central force must be the same; and, *secondly*, that the perpendicular to the tangent forms one side of a triangle similar to the triangle of which the differential of the radius vector is a side; the other side of the former triangle being the radius vector, the proportion of which to the force itself is the material point in all such inquiries. The difficulty of solving all these problems arises from the difficulty of obtaining simple expressions for those two lines, the perpendicular p and the radius of curvature R. The radius vector r being always $\sqrt{x^2+y^2}$ interposes little embarrassment; but the other two lines can seldom be concisely and simply expressed. In some cases the value of F, the force, by dr and dp may be more convenient than in others; because p may involve the investigation in less difficulty than R; besides that p^3 enters into the expression which has no differentials. But in the greater number of instances, especially where the curve is given, the formula $\frac{r}{p^3 \mathrm{R}}$ will be found most easily dealt with.

ii. We are next to consider the motion of bodies in conic sections which are given, and ascending or descending in straight lines under the influence of gravity; that is, the velocities and the times of their reaching given points, or

* Laplace (Méc. Cel. lib. xv. ch. i.) refers to this remarkable passage as the germ of Lagrange's investigations in the Berlin Mémoires for 1786.

their places at given times. This branch of the subject, therefore, divides itself into two parts; the one relating to motion in the conic sections, the other to the motion of bodies ascending or descending under the influence of gravitation.

In order to find the place of a revolving body in its trajectory at any given time, we have to find a point such that the area cut off by the radius vector to that point shall be of a given amount; for that area is proportional to the time. Thus, suppose the body moves in a parabola, and that its radius vector completes in any time a certain space, say in half a year moves through a space making an area equal to the square of D; in order to ascertain its position in any given day of that half year, we have to cut off, by a line drawn from the centre of forces, an area which shall bear to D^2 the same proportion that the given time bears to the half year, say 3 to m^2, or we have to cut off a section A S P = $\frac{3}{m^2} D^2$, A P being the parabola and S the focus. This will be done if A B be taken equal to three times A S, and B O being

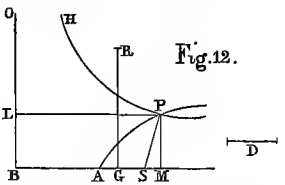


Fig. 12.

drawn perpendicular to A B, between B O, B A asymptotes, a rectangular hyperbola is drawn, H P, whose semi-axis or semi-parameter is to D in the proportion of 6 to m; it will cut the parabolic trajectory in the point P, required. For calling A M = x and P M = y and A S = a; then A B = $3a$ and $y \times (x + 3a)$ = half the square of the hyperbola's semi-axis, which axis being equal to $\frac{6D}{m}$, $y(x + 3a) = \frac{36D^2}{2m^2} = \frac{18D^2}{m^2}$,

or $y\left(\frac{x}{3}+a\right)=\frac{6\,D^2}{m^2}$, and $y\left(\frac{x}{6}+\frac{a}{2}\right)=\frac{3\,D^2}{m^2}$, or $y\times\left(\frac{2}{3}x-\frac{1}{2}\right.$ $\left.x+\frac{1}{2}a\right)=\frac{3\,D^2}{m^2}$. Therefore $\frac{2}{3}xy-\frac{1}{2}(x-a)y=\frac{3\,D^2}{m^2}$, and $\frac{2}{3}$, $AM\times PM=\frac{2}{3}xy$; and $\frac{1}{2}(x-a)y=\frac{1}{2}SM.PM=$ SMP; therefore the sector $ASP=\frac{3\,D^2}{m^2}$: so that the radius from the focus S cuts off the given area, and therefore P is the point where the comet or other body will be found in $\frac{3}{m^2}$ parts of the time.

If the point is to be found by computation, we can easily find the value of y by a cubic equation, $y^3+3a^2y=\frac{18\,a^2\,D^2}{m^2}$, and making $BL=y$, LP parallel to AM, cuts AP in the point P required. Sir Isaac Newton gives a very elegant solution geometrically by bisecting AS in G, and taking the perpendicular GR to the given area as 3 to 4 AS, or to SB, and then describing a circle with the radius RS; it cuts the parabola in P, the point required. This solution is infinitely preferable to ours by the hyperbola, except that the demonstration is not so easy, and the algebraical demonstration far from simple.

It is further to be observed, that the place being given, either of these solutions enables us to find the time. Thus, in the cubic equation, we have only to find $\frac{3\,D^2}{m^2}$. It is equal to $\frac{y^3+3a^2y}{6\,a^2}$; and as D^2 is the given integer, or period of *e.g.* half a year, the body comes to the point P in a time which bears to D^2 the proportion of unity to $\frac{6\,a^2\,D^2}{y^3+3a^2y}$.

iii. The next object of research is to generalise the preceding investigations of trajectories from given forces, and of motion in given trajectories, applying the inquiry to all kinds of centripetal force, and all trajectories, instead of confining it to the conic sections, and to a force inversely as the square of the distance.

We formerly gave the manner of finding the force from the trajectory in general terms, and showed how, by means of various differential expressions, this process was facilitated. It must, however, be remarked, that the inverse problem of finding the trajectory from the force, is not so satisfactorily solved by means of those expressions. For example, the most general one at which we arrived of $\frac{\sqrt{y^2+(x-a)^2}\times dx^2.dX}{2(y\,dx-(x-a)\,dy)^3}$ being put $=\frac{C}{y^2+(x-a)^2}$, or the force inversely as the square of the distance, presents an equation in which it may be pronounced impossible to separate the variables so as to integrate, at least while dX, the differential of $\frac{dy}{dx}$, remains in so unmanageable a form; for then the whole equation is $\frac{d^2y\,dx-d^2x\,dy}{2(y\,dx-(x-a)\,dy)^3}=\frac{C}{(y^2+(x-a)^2)^{\frac{3}{2}}}$, and thus from hence no equation to the curve could be found. It cannot be doubted that Sir Isaac Newton, the discoverer of the calculus, had applied all its resources to these solutions, and as the expressions for the central force, whether $\frac{r}{2p^3.r}$, or $\frac{dp}{p^3\,dr}$, or $-\frac{d^2x\sqrt{x^2+y^2}}{x}$ (in some respects the simplest of all, being taken in respect of dt constant, and which is integrable in

the case of the inverse squares of the distances, and gives the general equation to the conic sections with singular elegance), are all derivable from the Sixth Proposition of the First Book, it is eminently probable that Newton had first tried for a general solution by those means, and only had recourse to the one which he has given in the Forty-first Proposition when he found those methods unmanageable. This would naturally confirm him in his plan of preferring geometrical methods; though it is to be observed that this investigation, as well as the inverse problem for the case of rectilinear motion in the preceding section, is conducted more analytically than the greater part of the Principia, the reasoning of the demonstration conducting to the solution and not following it synthetically.

A is the height from which a body must fall to acquire the velocity at any point D, which the given body moving in the trajectory V I K (sought by the investigation) has at the corresponding point I; D I, E K, being circular arcs from the centre C, and C I = C D and C K = C E. It is shown previously that, if two bodies whose masses are as their weights descend with equal velocity from A, and being acted on by the same centripetal force, one moves in V I K and the other in A V C, they will at any corresponding points have

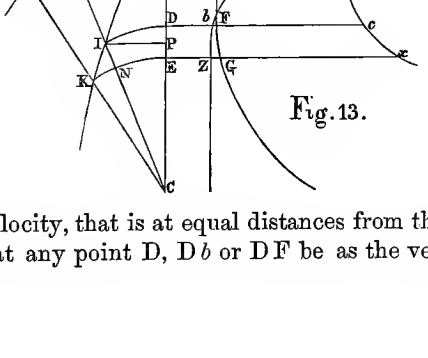


Fig. 13.

the same velocity, that is at equal distances from the centre C. So that, if at any point D, D b or D F be as the velocity at D

of the body moving in A V C, D b or D F will also represent the velocity at I of the body moving in V I K. Then take D F $= y$ as the centripetal force in D or I (that is, as any power of the distance D C, or $a - x$, V C being a, and C D, x) V D F L will be $\int y\,dz$. Describe the circle V X Y with C V as radius. Let V X $= z$, and Y X will be dz, and N K $= \frac{x\,dz}{a}$. Then I C K being as the time, and dt being constant, that triangle, or $\frac{\text{I C} \times \text{K N}}{2}$, is constant, and K N is as a constant quantity divided by I C, or as $\frac{\text{Q}}{x}$. If we take $\frac{\text{Q}}{x}$ to $\sqrt{\text{A V L B}}$ (proportioned to the force at any one point V and therefore given), as K N to I K, therefore this will in all points be the proportion; and the squares will be proportional, or $\int y\,dx$: $\frac{\text{Q}^2}{x^2}$ :: I K^2, or K N^2 + I N^2, to K N^2; and therefore $\int y\,dx - \frac{\text{Q}^2}{x^2}$: $\frac{\text{Q}^2}{x^2}$ :: I N^2, or $dx^2 : \frac{x^2\,dz^2}{a^2}$. Therefore $\frac{x\,dz}{a} = \frac{\text{Q}\,dx}{x\sqrt{\int y\,dx - \frac{\text{Q}^2}{x^2}}}$; and multiplying by x, $\frac{x^2\,dz}{a}$ (twice the sector I C K) $= \frac{\text{Q}\,dx}{\sqrt{\int y\,dx - \frac{\text{Q}^2}{x^2}}}$. Again $a\,dz : \frac{x^2\,dz}{a}$ :: $a^2 : x^2$; and $a\,dz = \frac{x^2\,dz}{a} \times \frac{a^2}{x^2} = \frac{a^2}{x^2} \times \frac{\text{Q}\,dx}{\sqrt{\int y\,dx - \frac{\text{Q}^2}{x^2}}}$ = twice the sector Y C X.

Hence results this construction. Describe the curve $a\,b$ Z, such that (D $b = u$) its equation shall be $u = \frac{\text{Q}}{2\sqrt{\int y\,dx - \frac{\text{Q}^2}{x^2}}}$, and the curve $a\,c\,x$ such that (D $c = \phi$) its equation may be

$\phi = \dfrac{Q a^2}{2 x^2 \sqrt{\int y\,dx - \dfrac{Q^2}{x^2}}}$. Then the differentials of the areas of these curves, or $u\,dx$ and $\phi\,dx$, being respectively $\dfrac{Q\,dx}{2\sqrt{\int y\,dx - \dfrac{Q^2}{x^2}}}$ and $\dfrac{Q a^2\,dx}{2 x^2 \sqrt{\int y\,dx - \dfrac{Q^2}{x^2}}}$, and those being equal to $\dfrac{x^2 dz}{2a}$ and $\dfrac{a\,dz}{2}$, or the sectors which are the differentials of the areas V I C and V X C, the areas themselves are equal to those areas; and therefore from V X C being given (if the area c D V a be found), and the radius C V being given in position and magnitude, the angle V C X is given; and from C X being given in position, and C V in magnitude and position, and also the area C I V, (if V D b a be found), the point I is found, and the curve V I K is known. This, however, depends upon the quantities made equal to u and ϕ severally being expressed in terms of x, for this is necessary in order to eliminate y from the equations to these curves; and then it is necessary to integrate these expressions; for else the angle V C X, and the curve V I K, are only obtained in differential equations. Hence Sir Isaac Newton makes the quadrature of curves, that is, first the integration of $\int y\,dx$, to eliminate y, and then the integration of the equations resulting in terms of u and x, ϕ and x respectively, the assumptions or conditions of his enunciation.—The inconvenience of this method of solving the problem gave rise to the investigations of Hermann and Bernoulli. The equation of the former, involving, however, the second differential of the co-ordinate, is to the rectangular co-ordinates; that of the latter is a polar equation, in terms of the radius vector and angle at the centre of forces.

To illustrate the difficulty with which this method of quadratures is applied, in practice—take the case of the centripetal force being inversely as the cube of the distance;

then $y = \frac{\mu}{x^3}$ and the curve B L F is quadrable. If we seek the circle V X Y by rectangular co-ordinates X O, O V, we find the equation to obtain O V = D in terms of x, is of the form

$$\frac{1}{2}\frac{a^2\,d\,\mathrm{D}}{\sqrt{2\,a\,\mathrm{D} - \mathrm{D}^2}} = \frac{\mathrm{Q}\,a^2\,d\,x}{2\,x^2\sqrt{\int y\,d\,x - \frac{\mathrm{Q}^2}{x^2}}}$$

$$= \frac{\mathrm{Q}\,a^2\,d\,x}{\sqrt{2}\,x\sqrt{2\,c\,x^2 - \mu - 2\,\mathrm{Q}^2}}$$

(c being the constant introduced by integrating $\int y\,d\,x$). Now there is no possibility of integrating these two quantities otherwise than by sines, and we thus obtain, nor can we do more, the following equation to D in terms of x;

$$\mathrm{C} - a^2 \text{ arc sin. } \frac{a - \mathrm{D}}{a} = \frac{\sqrt{2}\,.\,\mathrm{Q}\,.\,a^2}{\sqrt{\mu + 2\,\mathrm{Q}^2}} \times \text{arc cos. } \frac{\sqrt{\mu + 2\,\mathrm{Q}^2}}{\sqrt{2}\,c\,.\,x}.$$

And if we get D from this, in terms of cos. x, we have then to obtain P C by similar triangles, and from I P C being right-angled and I C = x, to obtain P I, in order to have the curve V I K.

But if we proceed otherwise, and instead of working by quadratures, take v the velocity of the body at I, or in the straight line at D, and make $\frac{c}{4}$ the area described in a second, and θ the angle V C I, we obtain as a polar equation to V I K, $d\theta = \frac{c\,d\,x}{x\sqrt{4\,x^2\,v^2 - c^2}}$ (x being in this case both C D and the radius vector). Then, to apply this general equation to the case of the centripetal force being as $\frac{1}{x^3}$, let the force at the distance 1 be put equal to unity, and supposing the velocity of projection to be that acquired in falling from an infinite height, the equation to the trajectory becomes

$$d\theta = \frac{c\,d\,x}{x\sqrt{4 - c^2}}, \text{ and integrating, } \theta = \frac{c}{\sqrt{4 - c^2}} \times \text{log. } \frac{x}{a}.$$

XII.

ATTRACTION OF BODIES; OF SPHERICAL AND NON-SPHERICAL SURFACES ANALYTICALLY TREATED.

i. The attraction exerted by spherical surfaces and by hollow spheres is first to be considered. If P be a particle situated anywhere within A B D C, and we conceive two lines A D, B C, infinitely near each other drawn through P to the surface, and if these lines revolve round a P b, which passes from the middle points a and b, of the small arcs D C, and A B, through P, there will two opposite cones be described; and the attraction of the small circles D C, A B upon P, will be in the lines from each point of those circles to P, of which lines C P, D P, are two from one cirle, and A P, B P, two from the other circle. Now, this attraction of the circle C D is to that of the circle A B, as the circle C D to the circle A B, or as CD^2 to AB^2 (the diameters), and by similar triangles $CD^2 : AB :: PC^2 : PA^2$. But by hypothesis, the attraction of C D is to that of A B as $AP^2 : PC^2$; therefore the attraction of D C is to the opposite attraction of A B as AP^2, to PC^2, and also as PC^2 to AP^2, or as $AP^2 \times PC^2$ to $AP^2 \times PC^2$, and therefore those attractions are equal; and being opposite they destroy one another. In like manner, any particle of the spherical surface on one side of P, acting in the direction of a P, is equal as well as opposite to the attraction of another particle acting on the opposite side, and so the whole action of every one particle is destroyed by

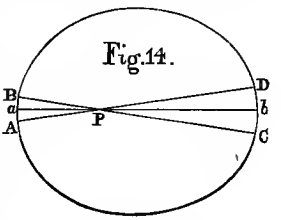


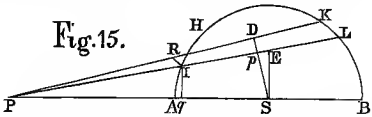
Fig. 14.

the opposite action of some other particle: and P is not at all attracted by any part of the spherical surface; or the sum of all the attractions upon P is equal to nothing. So of a hollow sphere; for every such sphere may be considered as composed of innumerable concentric spherical surfaces, to each of which the foregoing reasoning applies; and consequently to their sum.

We may here stop to observe upon a remarkable inference which may be drawn from this theorem. Suppose that in the centre of any planet, as of the earth, there is a large vacant spherical space, or that the globe is a hollow sphere; if any particle or mass of matter is at any moment of time in any point of this hollow sphere, it must, as far as the globe is concerned, remain for ever at rest there, and suffer no attraction from the globe itself. Then the force of any other heavenly body, as the moon, will attract it, and so will the force of the sun. Suppose these two bodies in opposition, it will be drawn to the side of the sun with a force equal to the difference of their attractions, and this force will vary with the relative position (configuration) of the three bodies; but from the greater attraction of the sun, the particle, or body, will always be on the side of the hollow globe next to the sun. Now the earth's attraction will exert no influence over the internal body, even when in contact with the internal surface of the hollow sphere; for the theorem which we have just demonstrated is quite general, and applies to particles wherever situated within the sphere. Therefore, although the earth moves round its axis, the body will always continue moving so as to shift its place every instant and retain its position towards the sun. In like manner, if any quantity of movable particles, thrown off, for example, by the rotatory motion of the earth, are in the hollow, they will not be attracted by the earth, but only towards the sun, and will all accumulate towards the side of the hollow sphere next the sun. So of any fluid, whether water or melted matter in the hollow, provided it do not wholly fill up the space, the whole of it will be accumulated towards the sun. Suppose it only

enough to fill half the hollow space; it will all be accumulated on one side, and that side the one next the sun; consequently the axis of rotation will be changed and will not pass through the centre, or even near it, and will constantly be altering its position. Hence we may be assured that there is no such hollow in the globe filled with melted matter, or any hollow at all, inasmuch as there could no hollow exist without such accumulations, in consequence of particles of the internal spherical surface being constantly thrown off by the rotatory motion of the earth.*

Fig. 15.



If A H K B be a spherical section (or great circle), P R K and P I L lines from the particle P, and infinitely near each other, S D, S E perpendiculars from the centre, and I q perpendicular to the diameter; then, by the similar triangles P I R, P p D, we find that the curve surface bounded by I H,

* The argument is here succinctly and popularly stated respecting the supposition of a hollow in the centre of the earth, and several steps are omitted. One of these may be mentioned in case it should appear to have been overlooked. Suppose a mass m detached from the hollow sphere M, and impelled at the same time with that sphere by an initial projectile force,—then its tendency would be to describe an elliptic orbit round the sun, the centre of forces, and if it were detached from the earth it would describe an ellipse, and be a small planet. But as the accelerating force acting upon it would be different from that acting on the earth, the one being as $\frac{S+M}{D^2}$, and the other as $\frac{S+m}{D^2}$ (D being the distance and S the mass of the sun), it is manifest that, sooner or later, its motion being slower than that of the hollow sphere, if m be placed in the inside, it must come in contact with the interior circumference of the sphere, and either librate, or, if fluid, coincide with it, as assumed in the text. Where parts of the spherical shell come off by the centrifugal force, of course no such step in the reasoning is wanted; nor is it necessary to add that neither those parts nor any other within the hollow shell can have any rotatory motion.

and formed by the revolution of I H K L I round the diameter A B, and which is proportional to $I H \times I q$, is as $\frac{I P^2}{P p \times P S}$; and if the attraction upon the particle P is as the surface directly, and the square of the distance inversely, or $\frac{1}{P I^2}$, that attraction will be as $\frac{1}{P p \times P S}$. But if the force acting in the line P I is resolved into one acting in P S and another acting in S D, the force upon P will be as $\frac{P q}{P I}$, or (because of the similar triangles P I Q, P S p) as $\frac{P p}{P S}$. The attraction, therefore, of the infinitely small curvilinear surface formed by the revolution of I H is as $\frac{P}{P p \times P S^2}$ or as $\frac{1}{P S^2}$; that is inversely as the square of the distance from the centre of the sphere. And the same may be shown of the surface formed by the revolution of K L, and so of every part of the spherical surface. Therefore the whole attraction of the spherical surface will be in the same inverse duplicate ratio.

In like manner, because the attraction of a homogeneous sphere is the attraction of all its particles, and the mass of these is as the cube of the sphere's diameter D, if a particle be placed at a distance from it in any given ratio to the diameter, as m. D, and the attraction be inversely as the square of that distance, it will be directly as D^3, and also as $\frac{1}{m^2 D^2}$, and therefore will be in the simple proportion of D, the diameter. Hence if two similar solids are composed of equally dense matter, and have an attraction inversely as the square of the distance, their attraction on any particle similarly placed with respect to them will be as their diameters. Thus, also, a particle placed within a hollow spheroid, or in a solid, produced by the revolution of an ellipsis, will not be

attracted at all by the portion of the solid between it and the surface, but will be attracted towards the centre by a force proportioned to its distance from that centre.

It follows from these propositions, *first*, that any particle placed within a sphere or spheroid, not being affected by the portion of the sphere or spheroid beyond it, and being attracted by the rest of the sphere, or spheroid in the ratio of the diameter, the centripetal force within the solid is directly as the distance from the centre;—*secondly*, that a homogeneous sphere, being an infinite number of hollow spaces taken together, its attraction upon any particle placed without it is directly as the sphere, and inversely as the square of the distance;—*thirdly*, that spheres attract one another with forces proportional to their masses directly, and the squares of the distances from their centres inversely;—*fourthly*, that the attraction is in every case as if the whole mass were placed in the central point;—*fifthly*, that though the spheres be not homogeneous, yet if the density of each varies so that it is the same at equal distances from the centre of each, the spheres will attract one another with forces inversely as the squares of the distances of their centres. The law of attraction, however, of the particles of the spheres being changed from the inverse duplicate ratio of the distances to the simple law of the distances directly, the attractions acting towards the centres will be as the distances, and whether the spheres are homogeneous or vary in density according to any law connecting the force with the distance from the centre, the attraction on a particle without will be the same as if the whole mass were placed in the centre; and the attraction upon a particle within will be the same as if the whole of the body comprised within the spherical surface in which the particle is situated were collected in the centre.

From these theorems it follows, that where bodies move round a sphere and on the outside of its surface, what was formerly demonstrated of eccentric motion in conic sections, the focus being the centre of forces, applies to this case of

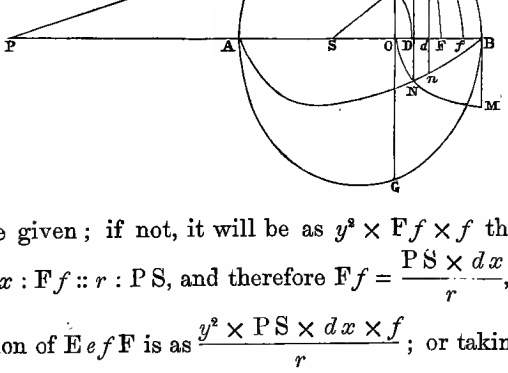
the attraction being in the whole particles of the sphere; and where the bodies move within the spherical surface, what was demonstrated of concentric motion in those curves, or where the centre of the curve is that of the attracting forces, applies to the case of the sphere's centre being that of attraction. For in the former case the centripetal force decreases as the square of the distance increases; and in the latter case thatforce increases as the distance increases. Thus it is to be observed, that in the two cases of attraction decreasing inversely as the squares of the central distance (the case of gravitation beyond the surface of bodies), and of attraction increasing directly with the central distance (the case of gravitation within the surface), the same law of attraction prevails with respect to the corpuscular action of the spheres as regulates the mutual action of those spheres and their motions in revolution. But this identity of the law of attraction is confined to these two cases.

Having laid down the law of attraction for these more remarkable cases, instead of going through others where the operation of attraction is far more complicated, Sir Isaac Newton gives a general method for determining the attraction whatever be the proportions between the force and the distance. This method is marked by all the geometrical elegance of the author's other solutions; and though it depends upon quadratures, it is not liable to the objections in practice which we before found to lie against a similar method applied to the finding of orbits and forces; for the results are easily enough obtained, and in convenient forms.

If A E B is the sphere whose attraction upon the point P it is required to determine, whatever be the proportion according to which that attraction varies with the distance, and only supposing equal particles of A E B to have equal attractive forces; then from any point E describe the circle E F, and another *e f* infinitely near, and draw E D, *e d* ordinates to the diameter A B. The sphere is composed of small concentric hollow spheres E *e f* F; and its whole attraction is equal to the sum of their attractions. Now that attraction

of $E e f F$ is proportional to its surface multiplied by $F f$, and the angle $D E r$ being equal to $D P E$ (because $P E r$ is a right angle by the property of the circle), therefore $E r = \frac{P E \times D d}{D E}$, and if we call $P E$, or $P F = r$, $E D = y$, and $D F = x$, $D d$ will be $d x$, and $E r = \frac{r d x}{y}$; and the ring generated by the revolution of $r E$ is equal to $r E \times E D$, or $r E \times y$; therefore this ring is equal to $r d x$, or the attraction proportional to the whole ring $E e$ will be proportional to the sum of all the rectangles $P D \times D d$, or $(a - x) d x$; that is, to the integral of this quantity, or to $\frac{2 a x - x^2}{2}$; which by the property of the circle is equal to $\frac{y^2}{2}$. Therefore the attraction of the solid $E e f F$ will be as $y^2 \times F f$, if the force of a particle $F f$

Fig 16.



on P be given; if not, it will be as $y^2 \times F f \times f$ that force. Now $d x : F f :: r : P S$, and therefore $F f = \frac{P S \times d x}{r}$, and the attraction of $E e f F$ is as $\frac{y^2 \times P S \times d x \times f}{r}$; or taking $f = r^n$ (as any power of the distance $P E$), then the attraction of $E e f F$ is as $P S . r^{n-1} y^2 d x$. Take $D N$ $(= u)$ equal to

P S . $r^{n-1} y^2$, and let B D = z, and the curve B N A will be described, and the differential area N D dn will be $n dz$ = (by construction) P S . $r^{n-1} y^2 dx$; consequently $u dz$ will be the attractive force of the differential solid E $e f$ F; and $\int u dz$ will be that of the whole body or sphere A E B, therefore the area A N B = $\int u dz$ is equal to the whole attraction of the sphere.

Having reduced the solution to the quadrature of A N B, Sir Isaac Newton proceeds to show how that area may be found. He confines himself to geometrical methods; and the solution, although extremely elegant, is not by any means so short and compendious as the algebraical process gives. Let us first then find the equation to the curve A N B by referring it to the rectangular coordinates D N, A D. Calling these y and x respectively, and making P A = b, A S (the sphere's radius) = a and P S, or $a + b$, for conciseness, = $\frac{f}{2}$. Then D E^2 = $2ax - x^2$; P E = $\sqrt{(b+x)^2 + 2ax - x^2}$ = $\sqrt{b^2 + 2(a+b)x}$ = $\sqrt{b^2 + fx}$; and D N = y = (by construction) $\frac{(a+b)(2ax - x^2)}{(b^2 + fx)^{\frac{n+1}{2}}}$, the attractive force of the particles being supposed as the $\frac{1}{n}$th power of the distance, or inversely as $(b^2 + fx)^{\frac{n}{2}}$. This equation to the curve makes it always of the order $\frac{n+3}{2}$.

If then the force is inversely as the distance, A N B is a conic hyperbola; if inversely as the square, it is a curve of the fifth order; and if directly as the distance, it is a conic parabola; if inversely as the cube, the curve is a cubic hyperbola.

The area may next be determined. For this purpose we have $\int y dx = \int \frac{f(2ax - x^2) dx}{2(b^2 + fx)^{\frac{n+1}{2}}}$. Let $2(af + b^2) = h$, this

integral will be found to be $\frac{1}{4(a+b)^2} \times \frac{h}{3-n} \times (b^2+fx)^{\frac{3-n}{2}}$ $- \frac{b^2}{1-n} \times (2a+b)^2 (b^2+fx)^{\frac{1-n}{2}} - \frac{(b^2+fx)^{\frac{5-n}{2}}}{5-n} + C$; and the constant C is $\frac{1}{4(a+b)^2} \times \left(\frac{b^{5-n}}{5-n} + \frac{(2a+b)^2 b^{3-n}}{1-n} - \frac{h}{3-n} b^{3-n} \right)$. This in every case gives an easy and a finite expression, excepting the three cases of $n = -1$, $n = 3$, and $n = 5$, in which cases it is to be found by logarithms, or by hyperbolic areas. To find the attraction of the whole sphere, when $x = 2a$, we have $\frac{1}{4(a+b)^2} \times \left(\frac{h}{3-n} (2a+b)^{3-n} - \frac{b^2}{1-n} \times (2a+b)^{3-n} - \frac{(2a+b)^{5-n}}{5-n} + \frac{b^{5-n}}{5-n} + \frac{b^{3-n}}{1-n} \times (2a+b)^2 - \frac{h b^{3-n}}{3-n} \right)$ for the whole area A N B, or the whole attraction. If P is at the surface, or A P $= b = 0$, and $n = 2$, then the expression becomes as a, that is, as the distance from the centre directly. We may also perceive from the form of the expression, that if n is any number greater than 3, so that $n - 3 = -m$, the terms b^{3-n} become inverted, and b is in their denominator thus: $\frac{(2a+b)^2}{(1-n) b^m}$. Hence if $n > 3$ and A P $= b = 0$, or the particle is in contact with the sphere, the expression involves an infinite quantity, and becomes infinite. The construction of Sir Isaac Newton by hyperbolic areas leads to the same result for the case of $n = 3$, being one of those three where the above formula fails. At the origin of the abscissæ we obtain, by that construction, an infinite area; and this law of attraction, where the force decreases in any higher ratio than the square of the distance, is applicable to the contact of all bodies of whatever form, the addition of any

other matter to the spherical bodies having manifestly no effect in lessening the attraction.

By similar methods we find the attraction of any portion or segment of a sphere upon a particle placed in the centre, or upon a particle placed in any other part of the axis. Thus in the case of the particle being in the centre S, and the particles of the segment R B G attracting with forces as the $\frac{1}{n}$th power of the distance S O or S I, the curve A N B will by its area express the attraction of the spherical segment, if D N or y be taken $= \frac{I O^2}{S D} = \frac{(x-a)^2 - c^2}{(x-a)^n}$, S O being put $= c$, and A D $= x$, and A S $= a$, as before; $\int y\,dx$ may be found as before by integrating $\frac{(x-a)^2\,dx - c^2\,dx}{(x-a)^n}$. The fluent is $\frac{(x-a)^{3-n}}{3-n} - c^2\frac{(x-a)^{1-n}}{1-n} + C$; and $C = \frac{2\,c^{3-n}}{n^2 - 4n + 3}$; and the whole attraction of the segment upon the particle at the centre S is equal to $\frac{a^{3-n}}{3-n} - \frac{c^2\,a^{1-n}}{1-n} + \frac{2\,c^{3-n}}{n^2-4n+3}$. Thus, if $n = 2$ the attraction is as $\frac{(a-c)^2}{a}$, or as O B^2 directly, and as S B inversely; and if $c = o$, or the attraction is taken at the centre, it is equal to a; and if the attraction is as the distance, or $n = 1$, then the attractive force of the segment is $\frac{1}{4}(a^2 - c^2)^2$.

ii. Attractions of non-spherical bodies. The attractions of two similar bodies upon two similar particles similarly situated with respect to them, if those attractions are as the same power of the distances $\frac{1}{n}$, are to one another as the masses directly, and the n^{th} power of the distances inversely, or the n^{th} power of the homologous sides of the bodies; and because

the masses are as the cubes of these sides, S and s, the attractions are as $S^3 \cdot s^n : s^3 \cdot S^n$, or as $s^{n-3} : S^{n-3}$. Therefore, if $n = 1$, the attraction is as $S^2 : s^2$; if the proportion is that of the inverse square of the distance, the attraction is as $S : s$; if that of the cube, the attraction is as 1 : 1, or equal; if as the biquadrate, the attraction is as $s : S$; and so on: and thus the law of the attractive force may be ascertained from finding the action of bodies upon particles similarly placed.

Let us now consider the attraction of any body, of what form soever, attracting with force proportioned to the distance towards a particle situated beyond it. Any two of its particles A B attract P, with forces as $A \times AP$ and $B \times BP$, and if G is their common centre of gravity, their joint attraction is as $(A + B) \times GP$, because BP, being resolved into BG and GP, and AP into AP and GP, and (by the property of the centre of gravity) $GP \times A = AP \times G$, therefore the forces in the line AP destroy each other, and there remain only $PG \times B$ and $PG \times A$ to draw P, that is $(A + B) \times PG$; and the same may be shown of any other particles C and the centre G′ of gravity, of A, C, and B, the attraction of the three being $(A + B + C) \times G'P$. Therefore the whole body, whatever be its form, attracts P in the line PS, S being the body's centre of gravity, and with a force proportional to the whole mass of the body multiplied by the distance PS. But as the mutual attractions of spherical bodies, the attraction of whose particles is as their distance from one another, are as the distances between the centres of those bodies, the attraction of the whole body A B C is the same with that of a sphere of equal mass whose centre is in S, the body's centre of gravity. In like manner it may be demonstrated that the attraction of several bodies A, B, C, towards any particle P, is directed to their common centre of gravity S, and is equal to that of a sphere placed there, and of a mass equal to the

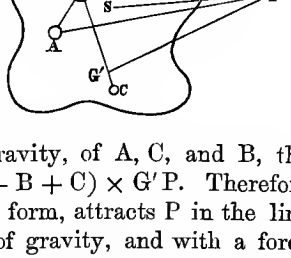


Fig. 47.

sum of the whole bodies A, B, C; and the attracted body will revolve in an ellipse with a force directed towards its centre as if all the attracting bodies were formed into one globe and placed in that centre.

But if we would find the attraction of bodies whose particles act according to any power n of the distance, we must, to simplify the question, suppose these to be symmetrical, that is, formed by the revolution of some plane upon its axis. Let A M C H G be the solid, M G the diameter of its extreme circle of revolution next to the particle P; draw P M and $p\,m$ to any part of the circle, and infinitely near each other, and take P D = P M, and P o = P m; D d will be equal to oM ($d\,n$ being infinitely near D N), and the ring formed by the revolution of M m round A B will be as the rectangle A M × M m,

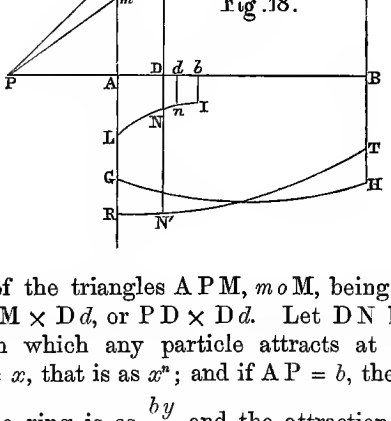


Fig. 18.

or (because of the triangles A P M, $m\,o$ M, being similar, and D d = $o\,m$) P M × D d, or P D × D d. Let D N be taken = y = force with which any particle attracts at the distance P D = P M = x, that is as x^n; and if A P = b, the force of any particle of the ring is as $\frac{by}{x}$, and the attraction of the ring, described by M m, is as $\frac{by}{x}$ × D d × P D, or as $b\,y\,d\,x$, and the

whole attraction of the circle whose radius is A M, being the sum of all the rings, will be as $b \int y\,dx$, or the area of the curve L N I, which is found by substituting for y its value in x, that is x^n. This fluent or area is therefore $= b \int x^n\,dx$ $= \frac{b\,x^{n+1}}{n+1} + C$; and $C = \frac{-\,b^{n+2}}{n+2}$. Also, making $P\,b = P\,E$ in order to have the whole area of L N I, which measures the attraction of the whole circle whose radius is F A, we have (x being $= P\,b = c$) $\frac{b\,c^{n+1}}{n+1} - \frac{l^{n+2}}{n+2}$ for that attraction. Then taking D N′ in the same proportion to the circle D E in which D N is to the circle A F, or as equal to the attraction of the circle D E, we have the curve R N T, whose area is equal to the attraction of the solid L H C F.

To find an equation to this curve, then, and from thence to obtain its area, we must know the law by which D E increases, that is, the proportion of D E to A D; in other words, the figure of the section A F E C B, whose revolution generates the solid.

Thus if the given solid be a spheroid, we find that its attraction for P is to that of a sphere whose diameter is equal to the spheroid's shorter axis, as $\frac{a\,.\,A^2 - D\,.\,L}{d^2 + A^2 - a}$ to $\frac{a^3}{3\,d^2}$, A and a being the two semi-axes of the ellipsoid, d the distance of the particle attracted, and L a constant conic area which may be found in each case; the force of attraction being supposed inversely as the squares of the distances. But if the particle is within the spheroid, the attraction is as the distance from the centre, according to what we have already seen.

Laplace's general formula for the attraction of a spherical surface, or layer, on a particle situated (as any particle must be) in its axis, is $\frac{2\pi u\,du}{r} \int f\,df \times \int df\,F$, in which f is the distance of the particle from the point where the ring cuts

the sphere, r its distance from the centre of the sphere, or the distance of the ring from that centre, du consequently the thickness of the ring, π the semicircle whose radius is unity, and F the function of f representing the attracting force. The whole attraction of the sphere, therefore, is the integral taken from $f = r - u$ to $f = r + u$, and the expression becomes $\frac{2\pi . u\,du}{r} + \int f\,df \times \int df\,\mathrm{F}$ with $(r + u) - (r - u)$, substituted for f, when f results from this integration. Then let $\mathrm{F} = \frac{1}{f^2}$ or the attraction be that of gravitation; the expression becomes $\frac{2\pi . u\,du}{r} \int f\,df \times \int \frac{df}{f^2} = \frac{2\pi . u\,du}{r} \times \frac{f^2}{2} \times -\frac{1}{f} = \frac{2\pi . u\,du}{r} \times -\frac{(r + u) - (r - u)}{2} = \frac{2\pi u\,du}{r} \times -u = -2\pi u^2\,du \times \frac{1}{r}$; and the coefficient of dr, taking the differential with r as the variable, is $+\frac{2\pi u^2\,du}{r^2}$; consequently the attraction is inversely as the square of the distance of the particle from the centre of the sphere, and is the same as if the whole sphere were in the centre.*

* Méc. Cel. liv. ii. ch. 2. The expression is here developed; but it coincides with the analysis in § 11.[1]

[1] This Tract and the last are both taken from the 'Analytical View of the Principia,' Lib. I.

XIII.

ADDRESS DELIVERED ON THE OPENING OF THE NEWTON MONUMENT AT GRANTHAM,

SEPT. 21, 1858.

To record the names, and preserve the memory of those whose great achievements in science, in arts, or in arms have conferred benefits and lustre upon our kind, has in all ages been regarded as a duty and felt as a gratification by wise and reflecting men. The desire of inspiring an ambition to emulate such examples, generally mingles itself with these sentiments; but they cease not to operate even in the rare instances of transcendent merit, where matchless genius excludes all possibility of imitation, and nothing remains but wonder in those who contemplate its triumphs at a distance that forbids all attempts to approach. We are this day assembled to commemorate him of whom the consent of nations has declared, that he is chargeable with nothing like a follower's exaggeration or local partiality, who pronounces the name of NEWTON as that of the greatest genius ever bestowed by the bounty of Providence, for instructing mankind on the frame of the universe, and the laws by which it is governed.

> "Qui genus humanum ingenio superavit, et omnes
> Restinxit; stellas exortus uti ætherius sol."—(*Luc.*)
>
> "In genius who surpassed mankind as far
> As does the mid-day sun the midnight star."—(*Dryden.*)

But though scaling these lofty heights be hopeless, yet is there some use and much gratification in contemplating by what steps he ascended. Tracing his course of action may

help others to gain the lower eminences lying within their reach; while admiration excited and curiosity satisfied are frames of mind both wholesome and pleasing. Nothing new, it is true, can be given in narrative, hardly anything in reflection, less still perhaps in comment or illustration; but it is well to assemble in one view various parts of the vast subject, with the surrounding circumstances whether accidental or intrinsic, and to mark in passing the misconceptions raised by individual ignorance, or national prejudice, which the historian of science occasionally finds crossing his path.

The remark is common and is obvious, that the genius of Newton did not manifest itself at a very early age. His faculties were not, like those of some great and many ordinary individuals, precociously developed. Among the former, Clairaut stands pre-eminent, who, at thirteen years of age, presented to the Royal Academy a memoir of great originality upon a difficult subject in the higher geometry; and at eighteen, published his celebrated work on Curves of Double Curvature, composed during the two preceding years. Pascal, too, at sixteen, wrote an excellent treatise on Conic Sections. That Newton cannot be ranked in this respect with those extraordinary persons, is owing to the accidents which prevented him from entering upon mathematical study before his eighteenth year; and then a much greater marvel was wrought than even the Clairauts and the Pascals displayed. His earliest history is involved in some obscurity; and the most celebrated of men has in this particular been compared to the most celebrated of rivers,* as if the course of both in its feebler state had been concealed from mortal eyes. We have it, however, well ascertained that within four years, between the age of 18 and 22, he had begun to study mathematical science, and had taken his place among its greatest masters; learnt for the first time the elements of geometry and analysis, and discovered a calculus which entirely changed the face of the science; effecting a complete revolution in that and in

* The Nile.

every branch of philosophy connected with it. Before 1661 he had not read Euclid; in 1665 he had committed to writing the method of Fluxions. At 25 years of age he had discovered the law of gravitation, and laid the foundations of Celestial Dynamics, the science created by him. Before ten years had elapsed, he added to his discoveries that of the fundamental properties of Light.—So brilliant a course of discovery, in so short a time changing and reconstructing Analytical, Astronomical, and Optical science, almost defies belief. The statement could only be deemed possible by an appeal to the incontestable evidence that proves it strictly true.*

By a rare felicity these doctrines gained the universal assent of mankind as soon as they were clearly understood; and their originality has never been seriously called in question. Some doubts having been raised respecting his inventing the calculus, doubts raised in consequence of his so long

* The birth of Newton was 25th Dec., 1642. (O. S.) or 5th Jan., 1643, (N. S.) In 1661, 5th June, he was entered of Cambridge, and matriculated 8th July. Before that time he had applied himself in a desultory way to parts of practical mechanics, as the movement of machines, and to dialling. As soon as he arrived at Cambridge he began to read 'Euclid,' and threw the book down as containing demonstrations of what he deemed too manifest to require proof. It is, therefore, probable that he had before meditated upon the position and proportion of lines, perhaps of angles. Upon laying aside 'Euclid,' he took up 'Descartes' Geometry,' then Kepler's Optics, which he speedily mastered, as he did a book on Logic, showing the College Tutor that he had anticipated his lessons. In 1663 and '64 he worked upon Series and the Properties of Curves. In summer, 1664, he investigated the quadrature of the hyperbolic area by the Method of Series which he had contrived. A paper in his handwriting dated 20th May, 1665, gives the method of Fluxions, and its application to the finding of tangents, and the radius of curvature. So that at this time the direct method at least was invented. Another paper also in his handwriting, Oct., 1666, gives its application to equations involving surds.

The Optical Lectures in 1669, '70 and '71, give the doctrine of Different Refrangibility.—In 1665 he formed the opinion of gravitation extending to the heavenly bodies, but was prevented from drawing the conclusion definitively, by the imperfect estimate of a degree as 60 miles, to which alone he had access. After 1670, when Picard showed it to be 69½ miles he resumed his demonstration, and found it exact.

withholding the publication of his method, no sooner was inquiry instituted than the evidence produced proved so decisive, that all men in all countries acknowledged him to have been by several years the earliest inventor, and Leibnitz, at the utmost, the first publisher; the only questions raised being, first, whether or not he had borrowed from Newton, and next, whether as second inventor he could have any merit at all; both which questions have long since been decided in favour of Leibnitz.*

But undeniable though it be that Newton made the great steps of this progress, and made them without any anticipation or participation by others, it is equally certain that there had been approaches in former times, by preceding philosophers, to the same discoveries. Cavalleri, by his 'Geometry of Indivisibles,' (1635,) Roberval, by his 'Method of Tangents,' (1637,) had both given solutions which Descartes could not attempt; and it is remarkable that Cavalleri regarded curves as polygons, surfaces as composed of lines, whilst Roberval viewed geometrical quantities as generated by motion; so that the one approached to the differential calculus, the other to fluxions; and Fermat, in the interval between them, came still nearer the great discovery by his determination of maxima and minima, and his drawing of tangents. More recently Schooten had made public similar methods invented by Hudde; and what is material, treating the subject algebraically, while those just now mentioned had rather dealt with it geometrically.† It is thus easy to per-

* Leibnitz first published his method in 1684; but he had communicated it to Newton in 1677, eleven years after the fluxional process had been employed, and been described in writing by its author.

† Cavalleri's 'Exercitationes Geometricæ' in 1647, as his 'Geometria Indivisibilis' in 1635, showed how near he had come to the calculus.—Fermat, however, must be allowed to have made the nearest approach; insomuch that Laplace and Lagrange have both regarded him as its inventor. He proceeds upon the position that when a Co-ordinate is a maximum or minimum, the equation, formed on increasing it by an infinitely small quantity, gives a value in which that small quantity vanishes. He thus finds the subtangent. But perhaps his most remark-

ceive how near an approach had been made to the calculus before the great event of its final discovery.

There had in like manner been approaches made to the law of gravitation, and the dynamical system of the universe. Galileo's important propositions on motion, especially on curvilinear motion, and Kepler's laws upon the elliptical form of the planetary orbits, the proportion of the areas to the times, and of the periodic times to the mean distances, and Huygens's theorems on centrifugal force, had been followed by still nearer approaches to the doctrine of attraction. Borelli had distinctly ascribed the motion of satellites to their being drawn towards the principal planets, and thus prevented from being carried off by the centrifugal force.*

Even the composition of white light, and the different action of bodies upon its component parts, had been vaguely conjectured by Ant. de Dominis, Archbishop of Spalatro, at the beginning, and more precisely in the middle of the seventeenth century by Marcus (Kronland of Prague), unknown to Newton, who only refers to the Archbishop's work; while the Treatise of Huygens on light, Grimaldi's observations on colours by inflexion as well as on the elongation of the image in the prismatic spectrum, had been brought to his attention, although much less near to his own great discovery than Marcus's experiment.†

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able approach to the calculus is the rule given to suppress all terms in which the square or the cube of the small quantity is found, because, it is said, those powers are infinitely small in comparison of the first power of the quantity. Thus calling that quantity e (or as we should say $d\,x$), he considers e^2 and e^3 (dx^2 and dx^3) as to be entirely rejected.—Hudde's letter to Schooten, 1658. Descartes' *Geom.* I. 507.

* Galileo's problem on the motion of bodies by gravity acting uniformly in parallel lines could have been no novelty to Newton; and Huygens's explanation of centrifugal tendency by the comparison of a stone's tendency to fly off when whirled round in a sling, is as correctly as possible that now received. But his theorems had been investigated by Newton several years before, as appears from a letter of Huygens himself.

† The Archbishop's explanation in 1611 of the rainbow, and his experiment to illustrate it by a thin glass globe filled with water and giving

But all this only shows that the discoveries of Newton, great and rapid as were the steps by which they advanced our knowledge, yet obeyed the law of continuity, or rather of gradual progress, which governs all human approaches towards perfection. The limited nature of man's faculties precludes the possibility of his ever reaching at once the utmost excellence of which they are capable. Survey the whole circle of the sciences, and trace the history of our progress in each, you find this to be the universal rule. In chemical philosophy the dreams of the alchemists prepared the way for the more rational though erroneous theory of Stahl: and it was by repeated improvements that his errors, so long prevalent, were at length exploded, giving place to the sound doctrine which is now established. The great discoveries of Black and Priestley on heat and aëriform fluids, had been preceded by the happy conjectures of Newton, and the experiments of others. Nay Voltaire* had well nigh

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colours by refraction, is remarkable; but far less so than Marcus's in 1648 on the 'Iris Trigonia,' as he calls the spectrum, and his observation of the colours not changing by a second refraction, so nearly approaching Newton's 'Experimentum Crucis.' It is best to mention this, because writers on the history of science have so often stated that nothing like a trace of the Newtonian doctrine of light can be found in the works of former observers. There is no appearance whatever of Newton having known Marcus's work.

* In his Prize Memoir we find (among many great errors chiefly arising from fanciful hypotheses) such passages as this, being an observation on one of his own experiments, 'Il y a certainement du feu dans ces deux liqueurs, sans quoi elles ne seraient point fluides;' and again, in speaking of the connexion between heat and permanent or gaseous elasticity, 'N'est ce pas que l'air n'a plus alors la quantité de feu nécessaire pour faire jouer toutes ses parties, et pour le dégager de l'atmosphère engourdie qui le renferme.' The experiments which he made on the temperature of liquids mixed together, led him to remark the temperature of the mixture as different from what might have been expected, regard being had to that of the separate liquids. Again, speaking of his experiments on the calcination of metals, 'Il est très possible que l'augmentation du poids soit venue de la matière répandue dans l'atmosphère; donc dans toutes les autres opérations par lesquelles les matières calcinées acquièrent du poids

discovered both the absorption of heat, the constitution of the atmosphere, and the oxidation of metals, and by a few more trials might have ascertained it.

Cuvier had been preceded by inquirers who took sound views of fossil osteology: among whom the truly original genius of Hunter fills the foremost place. The inductive system of Bacon, had been, at least in its practice, known to his predecessors. Observations and even experiments were not unknown to the ancient philosophers, though mingled with gross errors: in early times, almost in the dark ages, experimental inquiries had been carried on with success by Friar Bacon, and that method actually recommended in a treatise, as it was two centuries later, by Leonardo da Vinci; and at the latter end of the next century Gilbert examined the whole subject of magnetic action entirely by experiment. So that Lord Bacon's claim to be regarded as the father of modern philosophy rests upon the important, the truly invaluable, step of reducing to a system the method of investigation adopted by those eminent men, generalizing it, and extending its application to all matters of contingent truth, exploding the errors, the absurd dogmas, and fantastic subtleties of the ancient schools—above all, confining the subject of our inquiry, and the manner of conducting it, within the limits which our faculties prescribe.*

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cette augmentation pourrait aussi leur être venue de la même cause, et non de la matière ignée.' He had been experimenting with a view to try if heat had any weight. (*Acad. des Sciences*, 1737, *Prix. IV. p.* 169.)

* Friar Bacon's 'Opus Majus' was composed about the middle of the 13th century, certainly before 1267; and it contains, among other matters connected with experimental inquiry, a treatise expressly setting forth the advantages of that mode of philosophising. His aversion to the Aristotelian errors, and his departure from the whole philosophy of the times, was probably at the bottom of the charges of heresy under which he suffered cruel persecution for so many years.—Gilbert's Treatise, 'De Magnete et Corporibus Magneticis,' was published in 1600. It is entirely founded on experiments and observations, and is called by Lord Bacon "A painful and experimental work." Newton, who never alludes to Bacon, has been by some supposed not to have been acquainted with his writings. Sir D.

Nor is this great law of Gradual Progress confined to the physical sciences; in the moral it equally governs. Before the foundations of political economy were laid by Hume and Smith, a great step had been made by the French philosophers, disciples of Quesnay; but a nearer approach to sound principles had signalized the labours of Gournay, and those labours had been shared and his doctrines patronized by Turgot when Chief Minister. Again, in constitutional policy, see by what slow degress, from its first rude elements—the attendance of feudal tenants at their lord's court, and the summons of burghers to grant supplies of money—the great discovery of modern times in the science of practical politics has been effected, the Representative scheme, which enables states of any extent to enjoy popular government, and allows Mixed Monarchy to be established, combining freedom with order—a plan pronounced by the statesmen and writers of antiquity to be of hardly possible formation, and wholly impossible continuance.*—The globe itself as well as the science of its inhabitants, has been explored according to the law which forbids a sudden and rapid leaping forward, and decrees that each successive step, prepared by the last, shall facilitate the next. Even Columbus followed several successful discoverers on a smaller scale; and is by some believed to have had, unknown to him, a predecessor in the great exploit by which

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Brewster and others have peremptorily denied that his mode of inquiry was either suggested, or at all influenced by those writings. It is certain that neither he, nor indeed any one but Bacon himself, ever followed in detail the rules prescribed in the 'Novum Organum.'

* The opinion of Tacitus on this subject is well known. "Cunctas nationes et urbes populus, aut primores, aut singuli regunt. Delecta (some editions add consociata) ex his et constituta rei publicæ forma laudari facilius quam evenire; vel si evenit, haud diuturna, esse potest." (Ann. IV. 33.) Cicero, in his Treatise 'De Republicâ,' giving his opinion that the best form of government is that 'extribus generibus, regali, optimatum, et populari, modice confusa,' does not in terms declare it to be chimerical; yet he distinctly says in the same Treatise (II. 23) that liberty cannot exist under a king. Liberty, he says, consists "non in eo ut justo utamur domino sed ut nullo."

he pierced the night of ages, and unfolded a new world to the eyes of the old.

The arts afford no exception to the general law. Demosthenes had eminent forerunners, Pericles the last of them. Homer must have had predecessors of great merit, though doubtless as far surpassed by him as Fra Bertolomeo and Pietro Perugino were by Michael Angelo and Raphael. Dante owed much to Virgil; he may be allowed to have owed, through his Latin Mentor, not a little to the old Grecian; and Milton had both the Orators and the Poets of the ancient world, for his predecessors and his masters. The art of war itself is no exception to the rule. The plan of bringing an overpowering force to bear on a given point had been tried occasionally before Frederic II. reduced it to a system, and the Wellingtons and Napoleons of our own day made it the foundation of their strategy, as it had also been previously the mainspring of our naval tactics.

It has oftentimes been held that the invention of Logarithms stands alone in the history of science, as having been preceded by no step leading towards the discovery. There is, however, great inaccuracy in this statement; for not only was the doctrine of infinitesimals familiar to its illustrious author, and the relation of geometrical to arithmetical series well known; but he had himself struck out several methods of great ingenuity and utility, (as that known by the name of 'Napier's Bones,')—methods that are now forgotten, eclipsed as they were by the consummation which has immortalized his name.*—So the inventive powers of Watt, preceded as he was by Worcester and Newcomen, but more materially by Causs and Papin, had been exercised on some admirable con-

* 'The Rhabdologia,' was only published in 1617, the year he died; but Napier had long before the invention of logarithms used the contrivances there described. His 'Canon Mirificus' was only published by him in 1611: but it appears from a letter of Kepler that the invention was at least as early as 1594. The story of Longomontanus having anticipated him is a mere fable; but Kepler believed that one Byrge had at least come near the invention, and he had done much certainly upon natural sines. (Epist. Leips. 1718.)

trivances, now forgotten, before he made the step which created the engine anew, not only the Parallel Motion, possibly a corollary to the proposition on circular motion in the 'Principia,' but the Separate Condensation, and above all the Governor, perhaps the most exquisite of mechanical inventions; and now we have those here present who apply the like principle to the diffusion of knowledge, aware as they must be, that its expansion has the same happy effect naturally of preventing mischief from its excess, which the skill of the great mechanist gave artificially to steam, thus rendering his engine as safe as it is powerful.

The grand difference, then, between one discovery or invention and another is in degree rather than in kind; the degree in which a person while he outstrips those whom he comes after, also lives as it were before his age. Nor can any doubt exist that in this respect Newton stands at the head of all who have extended the bounds of knowledge. The sciences of Dynamics and of Optics are especially to be regarded in this point of view; but the former in particular; and the completeness of the system which he unfolded, its having been at the first elaborated and given in perfection,—its having, however, now stood the test of time, and survived, nay gained by the most rigorous scrutiny, can be predicated of this system alone, at least in the same degree. That the calculus, and those parts of dynamics which are purely mathematical, should thus endure for ever, is a matter of course. But his system of the universe rests partly upon contingent truths, and might have yielded to new experiments, and more extended observation. Nay, at times it has been thought to fail, and further investigation was deemed requisite to ascertain if any error had been introduced; if any circumstance had escaped the notice of the great founder. The most memorable instance of this kind is the dircrepancy supposed to have been found between the theory and the fact in the motion of the lunar apsides, which about the middle of the last century occupied the three first analysts of the age.* The error was

* D'Alembert, Clairaut, Euler.

discovered by themselves to have been their own in the process of their investigation; and this, like all the other doubts that were ever momentarily entertained, only led in each instance to new and more brilliant triumphs of the system.

The prodigious superiority in this cardinal point of the Newtonian, to other discoveries, appears manifest upon examining almost any of the chapters in the history of science. Successive improvements have by extending our views constantly displaced the system that appeared firmly established. To take a familiar instance, how little remains of Lavoisier's doctrine of combustion and acidification except the negative positions, the subversion of the system of Stahl! The substance having most eminently the properties of an acid, (chlorine,) is found to have no oxygen at all,* while many substances abounding in oxygen, including alkalis themselves, have no acid property whatever; and without the access of oxygenous or of any other gas, heat and flame are produced in excess. The doctrines of free trade had not long been promulgated by Smith, before Bentham demonstrated that his exception of usury was groundless; and his theory has been repeatedly proved erroneous on colonial establishments, as well as his exception to it on the navigation laws; while the imperfection of his views on the nature of rent is undeniable, as well as on the principle of population. In these, and such instances as these, it would not be easy to find in the original doctrines the means of correcting subsequent errors, or the germs of extended discovery. But even if philosophers finally adopt the undulatory theory of light instead of the atomic, it must be borne in mind that Newton gave the first elements of it by the well-known proposition in the eighth section of the second book of the 'Principia,' the scholium to that section also indicating his expectation that it would be applied to optical science;† while M. Biot has shown how the doctrine

* Recent inquiries are said to have shaken if not displaced Davy's theory of chlorine.

† The 47th prop. lib. II, has not been disputed except as to the sufficiency of the demonstration, which Euler questioned, but without adding the

of fits of reflection and transmission tallies with polarization, if not with undulation also.

But the most marvellous attribute of Newton's discoveries is that in which they stand out prominent among all the other feats of scientific research, stamped with the peculiarity of his intellectual character; they were, their great author lived before his age, anticipating in part what was long after wholly accomplished; and thus unfolding some things which at the time could be but imperfectly, others not at all comprehended; and not rarely pointing out the path and affording the means of treading it to the ascertainment of truths then veiled in darkness. He not only enlarged the actual dominion of knowledge, penetrating to regions never before explored, and taking with a firm hand undisputed possession; but he showed how the bounds of the visible horizon might be yet further extended, and enabled his successors to occupy what he could only descry; as the illustrious discoverer of the new world made the inhabitants of the old cast their eyes over lands and seas far distant from those he had traversed; lands and seas of which they could form to themselves no conception, any more than they had been able to comprehend the course by which he led them on his grand enterprise. In this achievement, and in the qualities which alone made it possible—inexhaustible fertility of resources, patience unsubdued, close meditation that would suffer no distraction, steady determination to pursue paths that seemed all but hopeless, and unflinching courage to declare the truths they led to how far soever removed from ordinary apprehension—in these characteristics of high and original genius we

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proof of its insufficiency, or communicating his own process. Cramer has done both, and his demonstration is given by Leseur and Jacquier, II. 364, together with another upon Newton's principle, but supplying the defects, by the able and learned commentators. The adherents, too, of the undulatory theory have always explicitly admitted the connexion between the Newtonian experiments and their doctrine.—See particularly Mr. Airy's very able Tracts—Thus, "Newton's rings have served in a great degree for the foundation of all the theories." S. 72, (p. 311, Edit. 1831.)

may be permitted to compare the career of those great men. But Columbus did not invent the mariner's compass, as Newton did the instrument which guided his course and enabled him to make his discoveries, and his successors to extend them by closely following his directions in using it. Nor did the compass suffice to the great navigator without making any observations; though he dared to steer without a chart; while it is certain that by the philosopher's instrument his discoveries were extended over the whole system of the universe, determining the masses, the forms, and the motions of all its parts, by the mere inspection of abstract calculations and formulas analytically deduced.*

The two great improvements in this instrument which have been made, the Calculus of Variations by Euler and Lagrange, the method of Partial Differences by D'Alembert, we have every reason to believe were known, at least in part, to Newton himself. His having solved an isoperimetrical problem (finding the line whose revolution forms the solid of least resistance) shows clearly that he must have made the co-ordinates of the generating curve vary, and his construction agrees exactly with the equation given by that calculus.†

* The investigation of the masses and figures of the planets from their motions by Newton—the discovery by Laplace of peculiarities in those motions never before suspected, a discovery made from the mere inspection of algebraical equations—without leaving their study—are as if Columbus had never left his cabin.

† The differential equation of the curve deduced by help of the calculus of variations is of this form:—

$$\frac{y\,d\,y^3}{} = c\,\frac{(d\,x^2 + d\,y^2)^2}{d\,x}.$$

Which may be reconciled with the equation in the commentary to the Schol. of Prop. XXXIV. lib. II.—If $p = \frac{d\,y}{d\,x}$, the equation becomes $y = \frac{c(1+p^2)^2}{p^3}$. T. Simpson, in his general solution of isoperimetrical problems ('Tracts,' 1757), gives a method which leads precisely to the above result derived from the calculus of variations, see p. 104. See, too, Emerson's 'Fluxions,' where we see his near approach to the calculus.

That he must have tried the process of integrating by parts in attempting to generalize the inverse problem of central forces before he had recourse to the geometrical approximation which he has given, and also when he sought the means of ascertaining the comet's path (which he has termed by far the most difficult of problems), is eminently probable, when we consider how naturally that method flows from the ordinary process for differentiating compound quantities by supposing each variable in succession constant; in short, differentiating by parts. As to the calculus of variations having substantially been known to him no doubt can be entertained.

Again, in estimating the ellipticity of the earth, he proceeded upon the assumption of a proposition of which he gave no demonstration (any more than he had done of the isoperimetrical problem) that the ratio of the centrifugal force to gravitation determines the ellipticity. Half a century later, that which no one before knew to be true, which many probably considered to be erroneous, was examined by one of his most distinguished followers, Maclaurin, and demonstrated most satisfactorily.

Newton had not failed to perceive the necessary effects of gravitation in producing other phenomena beside the regular motion of the planets and their satellites, in their course round their several centres of attraction. One of these phenomena, wholly unsuspected before the discovery of the general law, is the alternate movement to and fro of the earth's axis, in consequence of the solar (and also of the lunar) attraction combined with the earth's motion. This Libration, or Nutation, distinctly announced by him as the result of the theory, was not found by actual observation to exist till sixty years and upwards had elapsed, when Bradley proved the fact.*

* The Nutation, and by name, is given in Prin. Lib. III. prop. 21, the demonstration being referred to as in Lib. I. prop. 66, cor. 20. Clairaut, 'Princ. de Du Chatelet,' tom. II. p. 72, 73, refers to the same proposition. F. Walmsley, 'Phil. Trans.' 1746, has an excellent paper on Precession and Nutation, treated Geometrically. It is stated in Montucla, IV. 216,

The great discoveries which have been made by Lagrange and Laplace upon the results of disturbing forces, have established the law of periodical variation of orbits, which secures the stability of the system by prescribing a maximum and a minimum amount of deviation; and this is not a contingent but a necessary truth, deduced by rigorous demonstration as the inevitable result of undoubted data in point of fact—the eccentricities of the orbits, the directions of the motions, and the movement in one plane of a certain position. That wonderful proposition of Newton,* which with its corollaries may be said to give the whole doctrine of disturbing forces, has been little more than applied and extended by the labours of succeeding geometricians. Indeed, Laplace, struck with wonder at one of Newton's comprehensive general statements on disturbing forces in another proposition,† has not hesitated to assert, that it contains the germ of Lagrange's celebrated inquiry, exactly a century after the 'Principia' was given to the world. ‡

The wonderful powers of generalization, combined with the boldness of never shrinking from a conclusion that seemed the legitimate result of his investigations, how new and even startling soever it might appear, was strikingly shown in that memorable inference which he drew from optical phenomena, that the diamond is 'an unctuous substance coagulated;' subsequent discoveries having proved both that such substances are carbonaceous, and that the diamond is

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that Roemer had given some conjectural explanation of the phenomena of what he termed *vacillation;* but no date is assigned—Roemer died in 1710. In the same passage it is said that before Bradley's discovery, Newton had "suspected the nutation." He had deduced it from the propositions above referred to, and was considered so to have done by Clairaut. Bradley's paper was published in the 'Phil. Trans.' 1747; and it is not a little singular that he makes no mention at all of Newton.

* Lib. I. Prop. LXVI.

† The XVIIth's two last Corollaries.

‡ 'Mém de Berlin,' 1786, p. 253, is the memoir referred to by Laplace. The memoir is by Duval le Roi, but adopted by Lagrange as a supplement to his two memoirs, 1782 and 1784.

crystallized carbon; and the foundations of mechanical chemistry were laid by him with the boldest induction and most felicitous anticipations of what has since been effected.* The solution of the inverse problem of disturbing forces has led Le Verrier and Adams to the discovery of a new planet, merely by deductions from the manner in which the motions of an old one are affected, and its orbit has been so calculated that observers could find it—nay its disc as measured by them only varies one twelve-hundredth part of a degree from the amount given by the theory. Moreover, when Newton gave his estimate of the earth's density, he wrote a century before Maskelyne, by measuring the force of gravitation in the Scotch mountains, 1772, gave the proportion to water as 4·716 to 1—and many years after by experiment with mechanical apparatus Cavendish, 1798, corrected this to 5·48, and Baily more recently, 1842, to 5·66, Newton having given the proportion as between 5 and 6 times. In these instances he only showed the way and anticipated the result of future inquiry by his followers. But the oblate figure of the earth affords an example of the same kind, with this difference that here he has himself perfected the discovery, and nearly completed the demonstration. From the mutual gravitation of the particles which form its mass, combined with its motion round its axis, he deduced the proposition that it must be flattened at the poles; and he calculated the proportion of its polar to its equatorial diameter. By a most refined process he gave this

* 'Optics,' Book II. prop. 10.—It might not be wholly without ground if we conceived him also to have concluded, on optical grounds, that water has some relation to inflammable substances; for he plainly says that it has a middle nature between unctuous substances and others; and this he deduces from its refractive powers, though he gives other reasons in confirmation.—In the celebrated 31st Query, Book III. (p. 355), he plainly considers rusting, inflammation, and respiration, as all occasioned by the acid vapours in which he says the atmosphere abounds. In another place he treats of electricity as existing independent of its production or evolution by friction.—Black always spoke of that Query with wonder, for the variety of original views which it presents on almost every branch of chemical science.

proportion upon the supposition of the mass being homogeneous. That the proportion is different in consequence of the mass being heterogeneous does not in the least affect the soundness of his conclusion. Accurate measurements of a degree of latitude in the equatorial and polar regions, with experiments on the force of gravitation in those regions, by the different lengths of a pendulum vibrating seconds, have shown that the excess of the equatorial diameter is about eleven miles less than he had deduced it from the theory; and thus that the globe is not homogeneous: but on the assumption of a fluid mass, the ground of his hydrostatical investigation, his proportion of 229 to 230 remains unshaken, and is precisely the one adopted and reasoned from by Laplace, after all the improvements and all the discoveries of later times.—Surely at this we may well stand amazed, if not awe-struck.*—A century of study, of improvement, of discovery has passed away; and we find Laplace, master of all the new resources of the calculus, and occupying the heights to which the labours of Euler, Clairaut, D'Alembert, and Lagrange have enabled us to ascend, adopting the Newtonian fraction of one two-hundred-and-thirtieth, as the accurate solution of this speculative problem. New admeasurements have been undertaken upon a vast scale, patronised by the munificence of rival governments; new experiments have been performed with improved apparatus of exquisite delicacy; new observations have been accumulated, with glasses far exceeding any powers possessed by the resources of optics in the days of him to whom the science of optics, as well as dynamics, owes its origin; the theory and the fact have thus been compared and reconciled together in more perfect

* The wholly erroneous measurement of an arc by the two first Cassinis, (Dominic and James,) was supposed to prove the shortening of the degree towards the poles, in opposition to the Newtonian theory. But all doubt on the subject was set at rest by the admeasurement in Peru in 1735, and in Lapland in 1736, and in France more recently. But the error of Dominic and James Cassini was also corrected by the Cassini de Thury, who found that it had arisen from an imperfect measure employed.

harmony; but that theory has remained unimproved, and the great principle of gravitation, with its most sublime results, now stands in the attitude, and of the dimensions, and with the symmetry, which both the law and its application received at once from the mighty hand of its immortal author.

But the contemplation of Newton's discoveries raises other feelings than wonder at his matchless genius. The light with which it shines is not more dazzling than useful. The difficulties of his course, and his expedients, alike copious and refined, for surmounting them, exercise the faculties of the wise, while commanding their admiration; but the results of his investigations, often abstruse, are truths so grand and comprehensive, yet so plain, that they both captivate and instruct the simple. The gratitude, too, which they inspire, and the veneration with which they encircle his name, far from tending to obstruct future improvement, only proclaim his disciples the zealous, because rational, followers of one whose example both encouraged and enabled his successors to make further progress. How unlike the blind devotion to a master which for so many ages of the modern world paralysed the energies of the human mind!—

> "Had we still paid that homage to a name
> Which only God and nature justly claim,
> The western seas had been our utmost bound,
> And poets still might dream the sun was drown'd,
> And all the stars that shine in southern skies
> Had been admired by none but savage eyes."—(*Dryden.*)

Nor let it be imagined that the feelings of wonder excited by contemplating the achievements of this great man are in any degree whatever the result of national partiality, nor confined to the country which glories in having given him birth. The language which expresses her veneration is equalled, perhaps exceeded, by that in which other nations give utterance to theirs; not merely by the general voice, but by the well-considered and well informed judgment of the masters of science. Leibnitz, when asked at the royal table in Berlin his opinion of Newton, said that "taking mathe-

maticians from the beginning of the world to the time when Newton lived, what he had done was much the better half."—"The 'Principia' will ever remain a monument of the profound genius which revealed to us the greatest law of the universe," * are the words of Laplace. "That work stands pre-eminent above all the other productions of the human mind." † "The discovery of that simple and general law, by the greatness and the variety of the objects which it embraces, confers honour upon the intellect of man." ‡—Lagrange, we are told by Delambre, was wont to describe Newton as the greatest genius that ever existed; but to add how fortunate he was also, "because there can only once be found a System of the Universe to establish."§—"Never," says the father of the Institute of France, one filling a high place among the most eminent of its members—"Never," says M. Biot, "was the supremacy of intellect so justly established and so fully confessed."‖ "In mathematical and in experimental science without an equal and without an example; combining the genius for both in its highest degree." ¶ The 'Principia' he terms the greatest work ever produced by the mind of man, adding in the words of Halley that a nearer approach to the Divine nature has not been permitted to mortals.**—"In first giving to the world Newton's method of fluxions," says Fontenelle, "Leibnitz did like Prometheus—he stole fire from Heaven to teach men the secret." ††—"Does Newton," L'Hôpital asked, "sleep and wake like other men? I figure him to myself as of a celestial kind, wholly severed from mortality."

To so renowned a benefactor of the world, thus exalted to the loftiest place by the common consent of all men, one whose life without the intermission of an hour was passed in the

* 'Syst. du Monde,' V. 5. † Ib. V. 5.
‡ Ib. IV. 5. § 'Mém. de L'Instit.' 1812, p. XLIV.
‖ 'Journ. de Sav.' 1852, p. 135. ¶ 'Journ. de Sav.' 1852, p. 279.
** Ib. 1855, p. 552. "Nec fas est propius mortali attingere divos.'
†† 'Acad. des Sciences,' 1727.

search after truths the most important, and at whose hands the human race had only received good, never evil, those nations have raised no memorial which erected statues to the tyrants and conquerors, the scourges of mankind; whose lives were passed not in the pursuit of truth but the practice of falsehood; across whose lips, if truth ever chanced to stray towards some selfish end, it surely failed to obtain belief; who, to slake their insane thirst of power, or of pre-eminence, trampled on all the rights, and squandered the blood of their fellow-creatures; whose course, like the lightning, blasted while it dazzled; and who, reversing the noble regret of the Roman Emperor, deemed the day lost that saw the sun go down upon their forbearance, no victim deceived, or betrayed, or oppressed. That the worshippers of such pestilent genius should consecrate no outward symbol of the admiration they freely confessed, to the memory of the most illustrious of men, is not matter of wonder. But that his own countrymen, justly proud of having lived in his time, should have left this duty to their successors, after a century and a half of professed veneration and lip homage, may well be deemed strange. The inscription upon the Cathedral, masterpiece of his celebrated friend's architecture, may possibly be applied in defence of this neglect. "If you seek for a monument, look around."* If you seek for a monument lift up your eyes to the heavens which show forth his fame. Nor when we recollect the Greek orator's exclamation, "The whole earth is the monument of illustrious men," † can we stop short of declaring that the whole universe is Newton's. Yet in raising the Statue which preserves his likeness near the place of his birth, on the spot where his prodigious faculties were unfolded and trained, we at once gratify our honest pride as citizens of the same state, and humbly testify our grateful sense of the Divine goodness which deigned to bestow upon our race one so marvellously gifted to comprehend the works

* "Si monumentum quæris, circumspice." (On Wren in St. Paul's.)

† Pericles. (Thuc. II. 43.)

of Infinite Wisdom, and so piously resolved to make all his study of them the source of religious contemplations, both philosophic and sublime.

Besides the remarkable solution of T. Simpson, p. 53, his Tracts contain other singular anticipations. A very learned person (Mr Jerwood of Exeter) has pointed out a distinct anticipation of Lagrange's celebrated formula on the stability of the System. Nothing can be more delightful than contemplating the signal success of self-taught men, as Emerson was nearly—T. Simpson altogether—and both in humble circumstances.

NOTES.

Note I., pp. 18, 23.

The demonstration of the XXVIIIth Lemma, Principia, lib. I., has been generally admitted to be inconclusive; there being many curves which can be squared and rectified returning into themselves, and not falling within the exception in the Lemma, of curves having an oval, with infinite branches. Thus the whole of the figures whose equation is

$$y^m = n^m \left(x^{(n-1)^m}\right) \times (a^n - x^n),$$

are quadrable when m is an even number; for

$$\int y\,dx = \int n x^{n-1} (a^n - x^n)^{\frac{1}{m}}\,dx$$

is integrable, because the power of x without the radical sign is one less than the power within; and yet the curve can have no asymptote, because there is no divisor; while it is plain that the $\frac{1}{m}$ root of $a^n - x^n$ is impossible when either $+x$ or $-x$ is greater than a, n and m being both whole even numbers. Therefore the curve returns into itself; and, as $y = 0$ both when $x = 0$ and when $x = +a$, or $-a$; therefore the curve consists of two ovals touching at the origin. These are quadrable; for the integral $y\,dx$ is

$$C - \frac{m}{n(m+1)} (a^n - x^n) \cdot \frac{m+1}{m}.$$

The curve considered at length in Tract V. is another instance of the failure of the XXVIIIth Lemma; for that line continues through the cusps and returns into itself, though not, strictly speaking, an oval.—The cardiordeal also.

The demonstration given, instead of Sir I. Newton's, in Tract I., is not exposed to the other objections which have been made to the Newtonian demonstration; but it is equally liable to the objection now urged from the consideration of the equation to the class of curves whereof the lemniscata is one, and from the case of the curve described in Tract V., where the rectification is possible, as well as the quadrature. Perhaps we should extend the exception in the Lemma to curves which consist of two or more ovals touching each other, and to curves having cusps though without any infinite branches.

Note II., pp. 23, 74.

In the Encyc. xiii. p. 126, D'Alembert states Porism to be synonymous with Lemma in the ancient writers, but he adds that lemma is the only word used in modern times. His definition is not inaccurate as applied to lemma, a proposition of which we have need in order to pass to another more important; and on this he grounds his notion of porism, from πορος, passage. Under the word Poristique in another part of the Encyc., he gives a different definition of porism. Some authors, he says, call by this name the description "de la "manière de déterminer par quels moyens, et de combien, de "différentes façons un problème peut être résolu."

Nothing can be deduced from the Greek for *passage*, because the word πορισμα is plainly not derived from πορος, but from ποριζω, which rather sanctions the opinion connecting porism with corollary, than the opinion in the text of a transition from determinate to indeterminate. That the ancients sometimes used the word as synonymous with corollary there can be no doubt.

The subject has been handled incidentally by one of the most eminent geometricians of our day, M. Chasles, with his

wonted learning and perspicacity, in his celebrated treatise, *Geometrie Superieure*, Introd. p. xxi., and there is good reason to hope that a more full discussion will accompany his work lately announced as in preparation, the restoration of Euclid's three books (*Les trois Livres de Porismes d'Euclide rétablis pour la première fois*) M. Chasles puts in the front of his title that the restoration is effected after the notice and lemmas of Pappus, and conformably to the view of Simson, touching the form of the enunciation of the propositions. Nevertheless, his theory differs in some particulars from that of Simson. It has been highly gratifying to find that this great geometrician refers with approval to a porism in the First Tract (prop. vii.) which he considers to throw light upon one of the kinds of porisms described by Pappus as belonging to Euclid's Third Book. Perhaps he will cast an eye upon an illustration of the views entertained on this subject in Tract III.

It seems essential to the formation of a porism that there should be a transition from determinate to indeterminate, a change in the data which makes the problem indeterminate, and so capable of innumerable solutions. Take very simple and elementary cases.—Suppose the problem is to find in the diameter of a circle produced, a point such that the line drawn from it to a given point in the circumference shall have its square equal to the rectangle under the diameter produced, and the portion of it between the circle and the point to be found. Call the diameter a, the portion without the circle d, and the line cutting the circumference f, then $f^2 = (a + d)\, d$. Let y be the ordinate, and x the abscesse, to the given point in the circumference, then also

$f^2 = (d + x)^2 + a\,x - x^2 = d^2 + 2\,d\,x + a\,x$; and $d = \dfrac{a\,x}{a - 2\,x}$.

But if the point in the circumference is such that

$d + x = \dfrac{2\,(a\,x - x^2)}{a - 2\,x}$; then $\dfrac{a\,x}{a - 2\,x} + x = \dfrac{2\,(a\,x - x^2)}{a - 2\,x}$, and

$2\,a\,x - 2\,x^2 = 2\,a\,x - 2\,x^2$; or the point in the diameter produced is found whatever be the point in the circumference;

and also every point in the diameter produced gives a line cutting the circumference, and whose square is equal to the rectangle of the segments of the diameter.

So the data may be such as to render the solution impossible, and a change of these data making the solution indeterminate, a porism results. Thus, let it be required to draw from a given point in the diameter produced a line cutting the circumference, such that its square shall be equal to the rectangle contained by the whole line and that portion of it between the point and the ordinate to the point where the circumference is cut; then there is no such point of the diameter beyond the circle, because the square of the line drawn to cut the circumference must always be less than the rectangle under the segments of the diameter; but f^2 being as before $= d^2 + r\,dx + a\,x$, and being also = to $(a + d)\ (d + x)$ we have

$$d^2 + 2\,dx + a\,x = d^2 + d\,x + a\,x + a\,d, \text{ or } d\,x = a\,d;$$

and if $d = 0$, or the point is the extremity of the diameter, $f^2 = a\,x$, and any line drawn to any point of the circumference answers the conditions; so that when the problem is impossible, as well as when it admits of a determinate solution, a change of the data making it indeterminate will give rise to a Porism.

Again: an ellipse and a point without it being given, and a chord of the ellipse, let it be required to draw a straight line from the given point cutting the ellipse and the chord, so that it shall be divided by the ellipse and the chord in harmonical proportion; only one such line can be found, unless in the case of the chord being so situated that the tangents from the given point touch the ellipse at the extremities of the chord; and in that case every line drawn from the point, and cutting the ellipse and the chord, is divided in harmonical proportion.—Solving the problem algebraically; if the equation to the ellipse be $a^2y^2 + b^2(x - c)^2 = a^2b^2$; and that to the given chord $x = dy + n$; and that to the chord required $x = my$; and so in the quantity to be found we

obtain for the given chord $\frac{c^2 - a^2}{c} = d \times \frac{c^2 - a^2}{cm} + n$, and therefore $m = \frac{d(c^2 - a^2)}{c^2 - a^2 - nc}$; and thus we have the chord which it was required to find. But if the given chord be the line joining the points of contact of the tangents drawn to the ellipse from the given point; then $x = c - \frac{a^2}{c}$ $(= my)$, and therefore $d = 0$, and $n = \frac{c^2 - a^2}{c}$; so that $m = \frac{0}{0}$, and the problem becoming indeterminate, *any chord* answers the conditions of harmonic division.

It is remarkable with how great earnestness M. Chasles inculcates the advantage of studying the ancient writers, and how much he extols the preference which Sir I. Newton gives to synthetical demonstration conducted geometrically. We may be allowed, however, to question the degree in which he regards the Newtonian investigations as purely geometrical, and still more the assertion that they were conducted by the resources of the ancient geometry. The saying of Machin is well known that the Principia was algebra in disguise; and no one can doubt that the investigations were carried on by the resources of the calculus; the analysis being algebraical, and the composition or synthesis geometrical, at least in most instances.

Note III., p. 38.

Professor Playfair's enunciation of the principle is not quite satisfactory. "If," he says, "the motion which the particles "of a moving or a system of moving bodies, have at any "instant, be resolved into each two, one of which is the "motion which the particles had in the preceding instant, "then the sum of all these third motions must be such that "they are in equilibrium with one another."—(*Ed. Rev.* xi. 253.)

Mr. Ryley's statement of the principle is this:—After the enunciation given in page 38, line 21, "it would have re-"mained at rest,"—add, "Since these last forces mutually "destroy each other, and that the forces actually impressed "were compounded of them and of those (usually called "*effective*) which act in the direction the bodies really move "in, so that the force originally applied (usually called the "*impressed* force) is the result of these two forces, it follows "that the *effective* forces would, if they acted in the contrary "direction, exactly balance the *impressed* forces." He adds, that problems of dynamics are thus reduced to a general equation of equilibrium, and become statical.

Dr. Booth thus states it:—Since the *impressed* forces result in the *effective* forces, their differences or the *lost* forces must be zero—or equilibrate each other.

An excellent geometrician has observed, that "the prin-"ciple applies equally to the most elementary and the most "difficult problems; to the motion of a body down an inclined "plane and the vibration of a simple pendulum, or to the "theory of the radiation of heat and the vibrations of a chord "—two subjects of insuperable difficulty, to which D'Alembert "applied his new method of partial differences as well as his "principle, and which became remarkable in his hands, not "only for the solutions which he obtained, but also for the "manner of them." It was indeed his singular good fortune, by the further improvement of the calculus, to overcome the analytical difficulties into which the fecundity of his dynamical principle had led him.

Note IV.

There is not given among these Tracts the two papers on Light and Colours, inserted in the Phil. Trans. for 1796 and 1797, because objections have been made to the principal doctrines there maintained, upon the different reflexibility of the rays as supposed to be indicated in the colours which appear in the spectra made by reflexion from striated surfaces.

Those have been ascribed by most philosophers to other causes than a different reflexibility. But one proposition maintained in those papers has been generally admitted to be well founded, —the non-existence of reflexibility in the sense of Sir I. Newton; namely, the disposition to be reflected, and not transmitted. He conceived, that the most refrangible rays were also in this sense the most reflexible; and I ventured to make this objection, that his experiment introduces different refrangibility, being the reflexion from the base of a prism after the rays have been refracted, and when they are about to be refracted again. Professor Prevost, of Geneva, impugned my doctrine upon this subject in a paper which was inserted in the Phil. Trans. for 1799. But it is generally admitted that his objection applied rather to the course of my reasoning than to the support of the Newtonian doctrine, that is, to different reflexibility in the Newtonian sense; and this, there is reason to believe, has now been given up. Indeed, one circumstance appears sufficient to show that it can have no existence. If, instead of a prism which introduces different refrangibility into the experiment, we take an extremely thin plate of glass, and incline it in the rays of the spectrum, we find that there is no difference in the angle at which the different rays are reflected, instead of being transmitted. M. Arago, however, independent of this circumstance, considered the Newtonian different reflexibility as having been sufficiently disproved.

In these papers of 1796 and 1797 the different inflexibility of light was asserted, but not so fully proved as in these Tracts VII. and VIII. The experiments and observations in the Phil. Trans. for 1796 were made in 1794 and 1795, when the paper was sent to the Royal Society. There was an anticipation of Photography given in the copy of the paper first sent, but Sir C. Blagden considered that it referred rather to a subject of Art, and it was left out in the copy subsequently sent, and from which the paper was printed. According to the best of my recollection, it consisted of a remark on the effect of exposing a plate of ivory, stained with nitrate of silver, to the rays of the spectrum, and also on the effect

of exposing the plate to the rays passing through a very small hole into a dark room, and which form the image, more or less distinct, of external objects. It is unfortunate that this did not appear in the paper of 1796, because there can be little doubt that it would have led to making trials which must have ended in the discovery of the photographic process many years before it was eventually introduced.

Note V., p. 226.

The subsequent examination of the question touching the origin of aërolites appears to have thrown great light upon the subject, and may be said to have displaced the lunar theory. Laplace, Biot, and Poisson investigated the subject of the initial velocity required to project a body from the Moon to the Earth. Laplace made this 7,379 French feet in a second; Poisson, 7,123; Biot, 7,791; Olbers made it 7,780; and these numbers are without making any allowance for atmospheric resistance. But the mean velocity of aërolites is 114,000; and therefore the initial velocity is calculated at about 110,000, or fourteen times greater than Laplace's proportion, which he reckons at five or six times the velocity of a cannon ball. There is no ground for believing that any volcano exists in the Moon sufficiently active to exert this force. If one does exist it must be of double the force of any known on the earth. Furthermore the aërolites, for the most part, reach the earth moving in one direction; and there is no reason, on the lunar theory, why they should move in one direction rather than another.—The whole subject is treated with great clearness in Humboldt's 'Cosmos' (vol. i. p. 105), and more particularly in note 69, p. 383, of the admirable translation of that work by General Sabine, who has added some very valuable notes, especially at pp. 411, 454-457. Poisson (Mec. Annal. tom. i.) discusses the lunar theory of aërolites, and rejects it.

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